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ORIGINAL ARTICLES.

I.—Eminent Living Geologists.

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(WITH A PORTRAIT, PLATE XII.)

IT has frequently been asserted that the "born geologist"—as distinguished from the geologist made by education and training—owes his conception chiefly to *the formation* on which he happens to be born. Nor is it the beauty of the scenery and the attractiveness of firth and fell, mountain and glen, that usually give the impulse in the making of the geologist. It comes in most cases from *the fossils* he sees strewn around him in quarry or hillside—things that can be collected and fascinate the youthful mind even more than the rocks themselves. But whether the strata or the fossils are the stimulus required, it is beyond dispute that Yorkshire—in which both are conspicuous — takes a leading place in England as the birthplace of so many eminent geologists in the past century, and amongst them the subject of our present sketch worthily deserves to find a place.

George William Lamplugh was born at Driffield, East Yorkshire, on April 8, 1859, and here he spent his early years until he removed with his widowed mother to the coast at Bridlington when he was at the impressionable age of 13. It is scarcely possible that anyone having any sympathy with Nature should spend his youthful days upon the Yorkshire coast without becoming more or less of a geologist. Young Lamplugh soon began to collect the fossils from the Chalk and Drift, the latter deposit being a veritable open-air museum from the variety of its transported rocks and fossils. From the desire to know more about his collections he was led to the serious study of geology and to seek association with Yorkshire geologists, always a numerous and kindly folk. Amongst these he met with members of the Geological Survey working at the time in the district. Thus began a lasting friendship with the late J. R. Dakyns, with whom he spent some holidays in the field in various parts of the country. Circumstances compelled Lamplugh to enter early into business, but he resolutely determined to make science the serious object of his life, even if it did not procure for him the necessary means of livelihood.

Among the geological deposits on the Yorkshire coast that soon attracted Lamplugh's attention was the Boulder-clay series, to the

divisions of which, and in particular that known as the Bridlington Crag,[1] he devoted very careful work, and published the results in a series of papers, commencing in the GEOLOGICAL MAGAZINE for November, 1878 (pp. 509–17), in which the position of the shell-bearing beds in relation to the Boulder-clay, sands, and gravels is shown.

Besides the additions to the marine fauna made by Mr. Lamplugh (and identified by Dr. H. Woodward, F.R.S.), he records the discovery (in 1879, op. cit., p. 393) of a freshwater deposit rich in shells of *Limnæa peregra*, suggesting envelopment and transportation by the land-ice of both *freshwater* and marine deposits with the shells peculiar to each. He also read a paper in 1879 to the Yorkshire Geological and Polytechnic Society "On the Glacial Beds in Filey Bay" (the first of a series on kindred subjects communicated to this Society extending over many years).

It happened that the year 1881 was not only famous as the Jubilee of the British Association for the Advancement of Science, but the meeting was held in York, the city in which the Association was founded in 1831. The rally made by geologists, under the presidency of Professor (afterwards Sir A. C.) Ramsay, was truly remarkable, and the geologists of Yorkshire, amongst whom was G. W. Lamplugh (then 22), attended in force and gave it their whole-hearted support. Lamplugh's contribution to the splendid list of papers read in Section C was "On the Bridlington and Dimlington Glacial Shell-beds" (GEOL. MAG., 1881, pp. 535–46), with an excellent section of the cliff and lists of the Mollusca by Dr. J. Gwyn Jeffreys, of the Foraminifera by T. Rupert Jones, W. K. Parker, and Dr. H. C. Sorby. The recurrence of many papers on the Bridlington shell-beds is not merely due to their great importance, but to the fact that these beds are only occasionally seen, being almost constantly "masked" by masses of shingle and sand piled above them by the wind and tides, and moreover they are being gradually but *permanently* lost to sight by the construction of additional sea-walls to prevent the encroachment of the sea upon the cliffs. But for Lamplugh's long *residence on the spot*, their latest history would probably never have been written.

Lamplugh's first paper read before the Geological Society of London, in February, 1884, was on a recent exposure by storms of the shelly patches in the Boulder-clay at Bridlington in the winter of 1882–3. The mollusca, examined and determined by Dr. J. Gwyn Jeffreys, had been increased from 67 to 101, five of the additions being new to science; the Cirripedia were also determined by Mr. E. T. Newton and Foraminifera by Dr. Crosskey.

This year marked a determinative step in Lamplugh's life (he calls it his "wander-year"), for in it he started on a year's tour in North America for the purpose of increasing and enlarging his

[1] The history of the Bridlington Crag is given in a paper by the late Dr. S. P. Woodward in this journal, Vol. I, p. 49, 1864, which records details of the various early investigators and a list of the shells in this deposit compared with the Coralline Red and Norwich Crag, the Glacial deposits, and living species.

geological and general knowledge. The philosopher says "know thyself"; geologists say "know the world", and to do this a man must travel, travel, travel. He must possess also the trained eye and the retentive memory of the intelligent observer. After some study of drifts in the Eastern and North Central States, Lamplugh drifted gradually westward to the Pacific Coast, Vancouver Island, and Alaska. In winter he journeyed south to the Mexican border and as far as New Orleans. Afterwards he described a visit to the Muir Glacier in *Nature* and some features of glaciation observed in Vancouver Island in the Proceedings of the Yorkshire Geological and Polytechnic Society and in the Quarterly Journal for 1886.

On returning home Mr. Lamplugh took up with his accustomed activity his old geological exploration of the Yorkshire coast,[1] and especially devoted his attention to the subdivisions of the Speeton Clay. His notes on this formation in the Excursion Guide prepared for the London meeting of the International Geological Congress in 1888 brought him into personal association with several distinguished Continental geologists, who visited Speeton under his guidance, and led him to communicate an important paper on the subject to the Geological Society in March, 1889. This paper gave the results of a long series of observations made by him, during favourable opportunities, at the cliff foot and on the beach at Speeton from 1880 to 1889. As the result of his exhaustive labours he was able to show, on stratigraphical and palæontological evidence, that there is probably at Speeton a continuous series of clays from the Jurassic to the Upper Cretaceous, and that the deposition of these beds had gone on contemporaneously with the erosion of the beds inland.

This exploration of the Speeton Clay attracted the particular attention of the Russian geologist Professor Dr. Alexis P. Pavlow, of the University of Moscow. A critical study of the fossils by Professor Pavlow gave rise to a joint paper on the Speeton Clay and its Equivalents by A. Pavlow and G. W. Lamplugh.[2] In it the authors showed by comparative stratigraphy, and on the evidence of the Mesozoic Cephalopods from Russia, this, and other countries, the different "zones" into which the Speeton and Russian beds have been divided, and the actual sequence from the Kimmeridgian to the Aptian.

In the award to Mr. Lamplugh of the "Lyell Geological Fund" by the Council of the Geological Society in February, 1891, the President, Sir A. Geikie, referred to his valuable researches among the Glacial deposits of Yorkshire, and particularly to his investigation of the Speeton Clay, as a striking example of the results obtained by long and patient labours of an observer resident on the spot with unusual facilities to examine and study the beds.

In 1892 the opportunity so long awaited was afforded Lamplugh to join the Geological Survey as an Assistant Geologist, and, as

[1] He once described himself as "a coastguard" in the service of geological science.

[2] Published in the Bull. Soc. Imp. Nat. Moscou with 11 plates (Moscow, 1892); see also GEOL. MAG., 1892, pp. 422–6.

evidence of the high opinion held by the Director of his qualifications, he was sent to survey the Isle of Man, a task in which he was occupied for the greater part of the succeeding five years. The results of this period are embodied in his papers to the volumes of the Quarterly Journal and the Survey Memoir on the Isle of Man.

It is not often that one geological surveyor has the pleasure and satisfaction of seeing his name recorded as having written a memoir entirely by himself. The late Professor J. W. Judd when on the Survey many years ago claimed to have completed a whole English county, that of Rutland, but Mr. Lamplugh surveyed a whole island; nay, more, for was not Man a kingdom in itself up to 1765, when the Duke of Athol ceded his rights as Lord of Man to the Crown; but it still has its own Parliament (the House of Keys). Three-fourths of its whole area of 227 square miles (145,325 acres) is probably of Upper Cambrian age, whilst borings through Glacial drift have revealed a rock-floor of Triassic, Permian, and Lower Carboniferous strata below sea-level. Besides its valuable mines of silver-lead ore, its shell-marl and peat deposits have yielded many remains of the "Gigantic Irish Deer" (including an entire skeleton now set up in the Castle Rushen Museum, Isle of Man), which animal Mr. Lamplugh suggested may have crossed over to Man upon the ice towards the close of the Glacial period![1]

A brief leave of absence having been granted him, early in 1893 Mr. Lamplugh paid a flying visit to Arizona and the Pacific Coast of America and had a glimpse of the Grand Cañon of the Colorado.

Four years later, having been appointed Secretary of Section C (Geology), he attended the meeting of the British Association held in Toronto, Canada, and he joined an excursion across the Dominion to Vancouver Island under the guidance of Dr. G. M. Dawson, F.R.S., an account of which he published in *Nature* for November, 1897. In 1898 Mr. Lamplugh removed to Tonbridge to take part in the mapping of the Weald in conjunction with the examination of the borings and sinkings for coal then in progress in Kent (see memoir with Dr. Kitchin on Kent Mesozoic Rocks, 1911).

In 1901 the Council of the Geological Society awarded to him the Bigsby Medal (the "young man's medal"). In handing it to Mr. Lamplugh the President, Mr. Teall, said: "The Council feel that they are placing it in safe hands. You have done much, and they confidently expect that you will do more":—a trust which has since been honourably fulfilled by the recipient.

Having been appointed "District Geologist" in 1901, Mr. Lamplugh was sent to Dublin in charge of the Irish branch of the Geological Survey, in which post he remained until the Survey was transferred to an Irish department and placed under the supervision of Professor Grenville A. J. Cole, F.R.S., in 1905. During the period of his residence in Dublin Lamplugh superintended and took part in the mapping of the country around Dublin, Belfast, Cork, and Limerick, and issued four memoirs dealing with these areas.

[1] Another skeleton of *Cervus megaceros*, discovered in the Isle of Man in 1819, was presented to the Edinburgh Museum by the Duke of Athol. Many other remains of the same deer have been met with from 1798 onwards (see Geol. Surv. Mem., 1903, pp. 377-88).

In 1905 Mr. G. W. Lamplugh was elected a Fellow of the Royal Society, and in the same year he undertook, under the auspices of the British Association, the examination of the almost unexplored gorge of the Zambesi below the Victoria Falls, one of the grandest features of natural scenery to be met with on the African Continent. "It is difficult," says Mr. Lamplugh, "for anyone standing on the brink of the chasm, after having seen the placid flow of the Zambesi above the Falls, to believe that the fissure into which the river is so suddenly precipitated had been formed gradually by the action of the river itself, and not by some great convulsion during which the very crust of the earth was rent. The narrowness of the abyss, the strange zigzags along which the tumultuous waters rush, after their first great plunge, the mystery which has long surrounded the further course of the river after it swings away out of sight among its forbidding precipices, and the knowledge that the rocks across which it plunges are of volcanic origin are all factors that have aided the illusion." The conclusion arrived at by Mr. Lamplugh after examining the river carefully was quite in agreement with that already advanced by Mr. A. J. C. Molyneux that the prevalent idea of a sudden rent of the earth's crust was inadequate to explain the phenomena observed around the Falls, but was compatible with the view that the river has slowly sunk its channel into the hard rocks which have barred its passage seawards, while evidence afforded in other parts of the world sufficiently proves that canyons of even more impressive dimensions than the Zambesi have been carved out by the erosive agency of water acting through very long periods of time.[1]

In the following year Mr. Lamplugh was elected President of the Geological Section of the British Association in York and delivered an address on "Interglacial Problems".

Upon his return from Ireland he took charge of the survey of the Midland District (Nottinghamshire, etc.), and shared as writer and editor in the publication of several memoirs (see list). Subsequently he superintended the field-work in the North Wales district, the full results of which are not yet published.

In 1910 Mr. Lamplugh attended the meeting of the International Geological Congress at Stockholm; and previously to the meeting he joined with other noted geologists in an expedition to Spitsbergen, of which some account was contributed to *Nature* (December 1, 1910) and a description of a striking shelly moraine seen there to the Proceedings of the Yorkshire Geological Society for 1911.

After the retirement of Mr. Horace B. Woodward, F.R.S., in 1908 the administrative work of Assistant Director of the Survey was taken up by Dr. A. Strahan, F.R.S., until his promotion to the Directorship in 1914, when Mr. Lamplugh became Assistant Director.

In the latter year he made one of the distinguished band of geologists who represented our science on the occasion of the holding of the British Association in Australia (in August, 1914)

[1] A paper read before the British Association for the Advancement of Science, meeting in South Africa at Johannesburg, August 30, 1905. See also the *Official Guide to the Falls*, 1905, and the GEOLOGICAL MAGAZINE for December, 1905, pp. 529–32.

under exceptional facilities arranged by the Australian Governments. Not long after their arrival in the Commonwealth came the serious intelligence that war had been declared with Germany, a misfortune which overshadowed the programme and marred the closing stage of the meeting. Mr. Lamplugh was fortunately able, owing to the kindness of officials everywhere, to see much of the country, particularly in Western Australia, before the outbreak of war, under the guidance of Mr. Harry P. Woodward and Professor Woolnough.

Mr. Lamplugh is "no stranger in our midst", but is well known and highly esteemed in the scientific world, having been a Geological Surveyor for twenty-six years, and served upon the Councils of the Royal Society (1914–16), the Royal Geographical Society, and the Geological Society (1906–10, a Vice-President 1909–10, 1917), and is now its President (1918). As a Yorkshireman he keeps up his interest in all the amateur geological activities in the county. He is a past-President of the Yorkshire Naturalists' Union, the Hull Geological Society, and the Hertfordshire Natural History Society; and is an Honorary Member of the Rhodesian Scientific Association, the Yorkshire Philosophical Society, the Natural History and Antiquarian Society of the Isle of Man, and the Nottingham Naturalists' Society.

One who has worked with G. W. Lamplugh in the field and on the Survey and known him for some years writes:—

"If I were compelled to compress into three words my impression of Lamplugh's character, the ones I should choose would be courage, determination, and consistency—the courage which spurred him to break the current of his life and divert it to the work he loved and knew he could do best; the determination with which he has mapped out his career, passing through each objective to the next and never allowing an opportunity or experience to pass by unused; and the consistent high purpose which has guided the quality of his work, whether in the drifts, the Speeton clays, the Trias, or, that fool's paradise for geologists, the Isle of Man.

"These are the qualities which one sees in the field. A keen and accomplished observer as any glacial geologist must be or become, he has the elasticity of mind which enables him to turn to the discrimination of obscure igneous or metamorphic rocks, to the determination of ammonites or belemnites, or to the registering of those minute features of landscape which tell the history of physiography. Only here we must add the physical fitness for hard and steady work, and the disciplined imagination which have made the story of the Zambesi, or the glacial history of the Isle of Man, read like a fairy tale.

"But it is when the day's work is done and there 'creep out the little arts that please' that we discover the man of wide reading and liberal culture, of broad knowledge of places, men, and things, of deep convictions and serious thought. Then, if not before, we find the merciless critical faculty which takes nothing for granted, the insight which looks down into the heart of things, and the intolerance of sham and shoddy, which, seeking good in all, cannot shut its eyes to the evidence that all is not always for the best.

"Although he has undoubtedly read the hundred best books he has

by no means neglected the others, and, bringing to bear upon his great knowledge of literature, on its humane as well as its scientific side, a delicate perception and a nice and balanced judgment, he has become no mean judge of style and method. But the style that he appreciates must be the embroidery that accentuates worth and beauty and not that which is intended to hide deficiencies in both. It was no small triumph to have detected a new de Rougemont who had for the second or third time thrown dust into the eyes of those whose business it was to see clearly in matters of style.

"Keen as is his evaluation of books, his knowledge of men is not less well founded nor his judgment less sound. Having travelled far he has made a wide circle of acquaintances of varied sympathies and interests, and has met them under circumstances which favour close intimacy. To discuss men with him is as entertaining as to discuss books, for he has studied the man as well as his work, has seen the weak spots as well as the strength, and has the faculty of expressing his opinions with a slightly malicious but always good-natured humour which gives them a delightful if subacid flavour.

"If one might be allowed three more epithets they would be—as a geologist, *sound*; as a man, *human*; as a friend, *lovable*."

Mr. Lamplugh's large knowledge and wide experience in our science is always at the service of geologists who seek his kindly help. He is without pretence and rather too retiring, but—as he is only 59—that may be remedied as he grows older and has longer intercourse with his fellow-hammerers. We offer him our sincerest good wishes for his Presidency of the Geological Society, and he will also carry our warm regard with him for the term of his natural life.

H. W.

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1905. Geology of the Country around Cork and Cork Harbour.
Geology of the Country south and east of Devizes.
1907. Geology of the Country around Limerick.
1908. Geology of the Country between Newark and Nottingham.
Geology of the Country around Oxford.
1909. Geology of the Melton Mowbray district and S.E. Nottinghamshire.
1911. Geology of the Country around Ollerton.
1913. Geology of the Northern Part of the Derbyshire Coalfield, etc.
1915. Geology of the Country between Whitby and Scarborough, 2nd ed.

II.—NOTE ON A HYPERSTHENE ANDESITE FROM PITCULLO, FIFESHIRE.

By D. BALSILLIE, F.G.S., Chemistry Department, University of Edinburgh.

TWENTY-ONE years ago Dr. Flett in a valuable paper (Trans. Edin. Geol. Soc., vol. vii, 1897) described an exceedingly beautiful hypersthene andesite from the volcanic series of the Lower Old Red Sandstone at Dunyat in the Western Ochils. In the following brief communication I propose to give a short petrographic account of a strikingly similar rock that has occurred to me in the field in a more easterly portion of the same range of hills and which is of the same geological age.

In East Fife the volcanic rocks of the Lower Old Red Sandstone rise abruptly above the southern shores of the Firth of Tay. They consist here as elsewhere of lavas, tuffs, volcanic breccias, and agglomerates, with intervening belts of sediment that no doubt

marked intervals of quiescence more or less prolonged in the course of their volcanic history. The prevailing dip of the rocks is towards south-east, so that as we proceed in that direction progressively higher members of the sequence are encountered. As usual among the igneous products of this geological period, the most abundant extrusive types are andesites. More acid rocks, such as felsites, also occur, but there is no evidence to prove conclusively that in the area in question these are ever true lavas. (Reference may here be made to the salmon-pink felsite of Lucklaw Hill, which has hitherto been regarded as a lava. The enormous extension of this mass at right angles to the general strike of the rocks and the manner in which it sends ramifications into the andesites that surround it, especially on its northern boundaries, point only to one conclusion, viz. an intrusive origin.) The andesites vary greatly in their physical characters. Some are slaggy, having abundant mineral-filled vesicles, others being thoroughly compact with a platy system of joints and an occasional development of brecciation along their lower portions. These latter compact rocks afford excellent material for microscopic investigation, and as in many cases they have been laid bare in quarries for the provision of material to macadamize the public roads it is not difficult to collect representative examples of the fresher types of the district. It is in reference to a fortunate exposure in one of these artificial openings that this note has been written, and the remarks that follow are based entirely on the examination of rock specimens collected from one locality, viz. near Pitcullo in East Fife.

Inspection of a 1 inch to the mile topographic map of Fifeshire will show that on the main road between the county town of Cupar and Dundee there is, about 1 mile to the north-east of the little village of Dairsie, a farm steading named Muirhead, from which point a road runs west across the hills towards Craigsanquhar. Less than half a mile along this road from the junction just mentioned, and immediately on its right-hand side, a quarry has been opened among the lavas. The main rock of the quarry, which is situated at no great distance from the mansion house of Pitcullo, and is consequently designated the Pitcullo Quarry, is not of such character as to attract any special attention, but high on the worked face, on the side remote from the road, a rock of remarkable freshness and with a pitchstone-like resinous lustre occurs. It is coated externally with a light-brown crust, perhaps as much as a quarter of an inch in thickness, but beneath this weathered zone is very black and compact, and shows on a freshly broken surface abundant porphyritic crystals of a glassy felspar. There is no line of demarcation between this fresh rock and that lower in the quarry face, and the probability is that it is merely a modification of the rock occurring there.

Under the microscope confirmation of the fresh nature of the rock is immediately to be obtained. It consists of a remarkably homogeneous, brown in colour, isotropic groundmass, in which are disposed numerous porphyritic crystals of glassy felspar, orthorhombic and monoclinic pyroxenes, as well as irregular grains and granules of magnetite.

The porphyritic felspars, which in many cases are beautifully zoned, belong to the albite-anorthite series and exhibit albite, carlsbad, and pericline twinning. They are often elongated along the crystal axis *a*, but are sometimes also equidimensional on the second pinacoid. Examination by Fouqué's method shows that in sections normal to the bisectrix there is an angular divergence between the trace of the optic axial plane and (010) of 64°, the corresponding extinction angle on slices perpendicular to γ, measurement being made to the (001) cleavages, amounting to from 20° to 24°, these values agreeing well with an acid labradorite. Confirmation of this identification can be readily obtained from sections that exhibit both carlsbad and albite twinning by the well-known method of Michel Lévy, as also from slices cut normally to (010) and (001). The felspar phenocrysts are much more abundant than in the Dumyat rock, are equally fresh and unaltered, and as in its western counterpart the felspars here often contain glass inclusions and show evidence of resorption, though this phenomenon is not nearly so marked as in the rock described by Dr. Flett. Neither is there here the same indication of flow structure, the phenocrysts in the field of the microscope not showing the rough parallelism that immediately strikes one in the Dumyat rock. As noted, the felspar crystals are often elongated along the crystal axis *a*, and a single Baveno twin was observed cut almost normally to the bisectrices ***a***, the two halves giving angles of 60° and 61° respectively between the optic axial plane and (010). Each half was in addition twinned on the albite law, and an albite lamella in the one half was seen to be continued across the composition face and be prolonged in the other individual on the pericline law. This optical distinction could easily be made out under high magnification and making use of a thin gypsum plate.

The pyroxenes include, as has been said, both orthorhombic and monoclinic varieties. The former mineral, which predominates, is optically negative, markedly pleochroic, and may safely, I think, be taken as hypersthene. The crystal outlines are well-marked sections transverse to the crystal axis *c*, being rectangular in form, with the prism faces occurring as mere truncations of the corners. Such sections are invariably traversed by a set of irregular cracks, in addition to which there is a well-developed prismatic cleavage. A pinacoidal cleavage I did not observe. The hypersthenes are usually quite fresh and have a tendency to occur as little coteries of small crystals, sometimes in association with the monoclinic pyroxene or in other places along with the felspars. The pleochroism is as follows: *X* pale reddish-brown, *Y* pale yellow, *Z* green.

The monoclinic pyroxene is at once to be distinguished from the hypersthene by its oblique extinctions and higher interference colours. The dark borders that were noted by Dr. Flett to surround the augites in the Dumyat rock do not occur here. The crystal outlines are but ill defined, and twinning occurs on one or more laws that I did not determine. The mineral is not pleochroic, and on the prismatic zone of faces exhibits very often roughly parallel cleavage traces. The angle between the bisectrix Z ($=\gamma$) and (100) was

found to amount from 40° to 45°, this corresponding with augite, which I therefore take to be the monoclinic pyroxene present.

Magnetite occurs disseminated as quadrangular and irregular grains throughout the rock, and is frequently enclosed in the pyroxenes. Apatite such as occurs in the Dumyat and Cheviot andesites I did not observe.

The groundmass of the Pitcullo rock examined under low powers appears remarkably isotropic, but is resolvable under higher magnification to a mass of little felspar crystals that lie in an ultimate base of pale-brown or colourless glass with globulites. In addition to the felspars, which are twinned on the albite law and have only a small extinction angle, a second generation of pyroxenes also occurs. Magnetite, too, is an abundant constituent.

In sections of the Dumyat rock a number of lighter areas made up of minute microlites and having a darker periphery were observed by Dr. Flett. In their most perfect form these occur "as rounded bodies which have some resemblance to spherulites". Such circular aggregations of microlites I have not noted in the Pitcullo rock, though irregular areas of a similar kind, and often with a darker border, not uncommonly occur. Comparison, however, with hand-specimens of the typical kugel andesite from Bath, Hungary, convinces me that it would be quite unwise to describe kugel structure as occurring in the Pitcullo rock. The Hungarian rocks have had a sort of pseudo-amygdaloidal structure conferred upon them by the physically separable character of their kugels. This certainly is not the case in the rock from East Fife.

The rock that has now shortly been described is by far the freshest typical andesite that I know in the district in which it occurs. It is not, be it at once said, quite so glassy as the exceptionally fresh rock from Dumyat, but in the best specimens is only a little less so.

The bulk of the material in the quarry consists, as has been indicated, of hypersthene andesite, but is really a rock quite different in appearance from that described in the foregoing notes. It is grey in colour, is obviously in a much less fresh condition, and suggests in no way, macroscopically at all events, relationship with the fresh "pitchstone" that comes on top of it. Notwithstanding this physical distinction, both appear to have belonged to the same parent mass. Under the microscope the felspars can be seen to be equally clear and glassy and to have an identical composition. Both orthorhombic and monoclinic pyroxenes occur, as in the unaltered rock, the former, however, now only represented by pseudomorphs made up of strongly absorbing fibres. Impregnations and veins of iron oxide are frequent, obscuring totally in some slices the real nature of the rock that carries them. The base is much less glassy, and the felspars of the second generation are larger. It will therefore be seen that any distinction that occurs may only be due to differences in the cooling history of the two portions of the rock and to the fact that the glassy rock by virtue of its compactness has had conferred upon it a higher degree of resistance to the agencies of secondary change.

Being interested as to the chemical nature of the unaltered andesite, Mr. R. K. S. Mitchell, one of the senior workers in this laboratory, kindly undertook to carry out for me a determination of the silica and bases present, which he did under the supervision of an exceedingly skilled analyst, my friend Dr. Sidney A. Kay. Mr. Mitchell's figures are as follows:—

SiO_2	61·37
Al_2O_3	18·80
Fe_2O_3	5·46
MnO	—
CaO	5·62
MgO	1·88
Na_2O	3·83
K_2O	2·05
	99·01

Comparison of these results with the analyses given by Sir Jethro Teall in his valuable papers on the Cheviot andesites (Geological Magazine, 1883) will at once show in what striking fashion there is chemical similarity between the Old Red "pitchstone porphyrites" of the borders and the example described above from East Fife.

In conclusion, it is my duty to express very cordial thanks to Professor James Walker, F.R.S., for having added to the equipment of this Department several items of optical apparatus which have enabled me to carry out the foregoing mineral determinations.

III.—The Thickness of the Zone of *Belemnitella mucronata* in the Isle of Wight.

By R. M. Brydone, F.G.S.

In 1908 Dr. Rowe published an account of the Chalk of the Isle of Wight[1] with a zonal map in which the zone of *Belemnitella mucronata* was shown as entirely absent at some points but generally present in substantial thickness. At the two ends of the island actual measurements were made, of 150 feet at Culver Down and 475 feet at the Needles. How much of the latter thickness was measured in the cliffs and how much is made up of somewhat less satisfactory measurements at low tide on weed-covered reefs is not clear, but at any rate there must be well over 300 feet in the cliffs. This figure is sufficient to show how great a ravining of the pre-Tertiary surface of the chalk would be required for the repeated disappearances of the zone as mapped. It seems justifiable to pay some critical attention to the evidence on which such a state of things is alleged.

It will be found on reference to the map in question that there are five points at which the zone of *Belemnitella mucronata* is represented as disappearing completely, while in between them it swells out each time to a very substantial thickness. (I do not include the

[1] "The Zones of the White Chalk of the English Coast. V. The Isle of Wight": Proc. Geol. Assoc., vol. xx, pt. iv, p. 209.

case at the east end of Tapnell Down, as this is probably due to the combined effect of an imperfect junction between the sections and of exigencies of space leading to the exclusion of part of the chalk area.) Of these five cases, two appear to be arrived at by assumption only, but three are connected with palæontological evidence from sections and will be considered first.

The most interesting of these is undoubtedly the one just west of Freshwater by pit 11 (Rowe), for, as Dr. Rowe points out, it is but 2 miles from Alum Bay with its great thickness of *mucronata* chalk. The palæontological evidence given by Dr. Rowe is that the shells in the pit are white, and that two examples of *Echinocorys* of *quadratus* type were found. The former statement does not seem to have any bearing on the question unless it is established that no white shells are to be found in the lower *mucronata* chalk of the Isle of Wight. I know of no authority for such a proposition, and it is not in accord with any experience of mine, although Mr. Griffith and I long ago noted redness in the shells as a feature of the *upper* beds of the *mucronata* chalk. The phrase "*Echinocorys* of *quadratus* type" conveys no meaning to me. No shape of *Echinocorys* typical of upper *quadratus* chalk has, so far as I know, been defined in any way by Dr. Rowe, much less one which while typical of *quadratus* chalk cannot be found in low *mucronata* chalk, and only such a form would be of any value for assigning a section to the former zone to the exclusion of the latter. Even if such a form had been defined and definitely recognizable specimens found in this pit they would prove very little. All probability is in favour of *Echinocorys* recorded from this pit having been found on the extensive talus, the surface of which is obviously derived from the chalk high up at the back of the pit. In ordinary horizontal or slightly inclined chalk the identification of the *quadratus* zone at the top of a pit close to the Tertiary boundary would afford a strong presumption that no *mucronata* chalk was preserved at that point. But the Isle of Wight chalk is nearly vertical, and therefore the identification of the *quadratus* zone at the top of this pit—as to the accuracy of which I do not suggest any question, it having also been made by Mr. Griffith and myself many years ago—leaves the whole thickness between the back of the pit and the Tertiary boundary, a distance of some 120 feet, corresponding to a thickness of at least 100 feet of chalk, open to reference to either the *quadratus* zone or the *mucronata* zone. To carry the matter a step further, even if an absolutely decisive *Echinocorys* were found at the most northerly point where chalk is still exposed in the pit, there would still remain between that point and the Tertiary boundary some 30 feet of chalk, and this could not be ruled out of the *mucronata* zone on any palæontological ground.

As a matter of fact, there is some slight positive ground for holding the view that the *mucronata* zone is exposed within the pit. There is now a sideway extension, which I do not remember in the nineties, cut parallel with and close to the footpath, and some of the chalk exposed here is gritty and hard, contrasting rather sharply with the soft and fine-grained chalk at the back of the pit. I have

recorded precisely this contrast between the *quadratus* and *mucronata* chalk in Portsdown as separated on palæontological evidence.[1]

The second point at which palæontological grounds are quoted in support of the view that the *mucronata* chalk has been wholly removed is pit No. 3 (Afton Down). Here the position corresponds almost exactly with that at pit No. 11. There is a pit in which the presence of the *quadratus* zone is established by palæontological evidence, as Mr. Griffith and I recognized in the nineties; but the principal face and talus, from which this recognition was made, are at the back of the pit, some 80 feet from the Tertiary boundary. The chalk towards the mouth of the pit seems of different character from that at the back, and in it I have found what appears to be a broken specimen of *Thecidea Brydonei*, which is only known from the basal *mucronata* chalk of Portsdown. Here, again, there is nothing in the evidence cited by Dr. Rowe to warrant the statement that the *quadratus* zone extends to the mouth of the pit, much less to the Tertiary boundary; and there is some suggestion of evidence to the contrary.

As zonal indications are so scarce, it is perhaps worth mentioning that it was in this pit that I found the type of *Membraniporella Gabina*[2] many years ago on the talus, but in what part of the pit I cannot say for certain. I naturally attributed it to the zone undoubtedly represented in the pit, that of *A. quadratus*. At the time of describing the species I had only two other specimens, one a very doubtful one from the floor of this same pit near the mouth and one from the zone of *B. mucronata* at Portsdown. I have since recognized several other specimens all from indisputable *mucronata* chalk either in the Isle of Wight or at Portsdown. I think there is some justification for the view that this species, which has never yet been found in undoubted *quadratus* chalk, actively as the top beds of that zone have been exploited in Hants, is restricted to the *mucronata* zone, and has only been found in this pit because chalk of that zone is or has been exposed in it.

The third place at which the zone of *B. mucronata* is represented on palæontological evidence as entirely removed is at Ryde Waterworks (pit No. 45). Here we may remark, as in the two previous cases, that the critical section does not extend to the Tertiary boundary by some 90 feet, and cannot therefore afford evidence as to the nature of the chalk in contact with the Tertiaries. Even for the alleged *quadratus* horizon of the section itself the palæontological evidence given by Dr. Rowe is very scanty and purely negative, and scanty negative evidence can hardly form a satisfactory basis for the assertion of an exceptional state of things. In this case again there are grounds for doubting the reference of, at any rate, the whole of the section to the *quadratus* zone. The barrenness of the chalk is not in the least exaggerated by Dr. Rowe, but I have found on the talus below the section a small damaged *Echinocorys* likely to be the

[1] *The Stratigraphy of the Chalk of Hants*, p. 8 (London, Dulau & Co., 1912).

[2] Geol. Mag., 1917, p. 494, Pl. XXXII, Fig. 8.

var. *subconicus* characteristic of the *mucronata* zone. If this zone occurs at all in the section, there would be room for a minimum thickness of some 80 feet of it.

The two places at which no palæontological grounds are given for cutting out the *mucronata* zone altogether are at Burnt House (north of Garretts) and at the west end of Bembridge Down. It will be obvious on inspection that these disappearances of the zone are due solely to the thicknesses assigned to the other Upper Chalk zones exhausting all the available space between the Chalk Rock and the Tertiary boundary as mapped. These thicknesses appear at the points in question to be purely arbitrary; indeed, at the end of Bembridge Down the *quadratus* zone seems to have been deliberately given a special local increase of thickness but for which some *mucronata* chalk must have been shown at that point. Unexpected results obtained by such free methods of mapping seem to be much in need of testing before any authority is claimed for them. The prospect of being able to apply any test (except by making a special excavation) are naturally very small if Dr. Rowe found no exposure

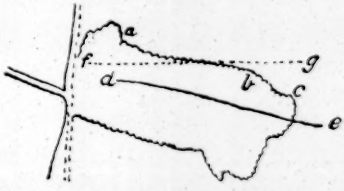

FIG. 1.—Tracing of a very small portion of a folding map (plate D), marked on map "Garretts to Arreton Down" as "Downend Chalk Pit (21)" on Gallows Hill. Illustrating Dr. Arthur W. Rowe's paper on "Zones of the White Chalk of the English Coast", part v, "The Isle of Wight" (see Proc. Geol. Assoc., vol. xx, pt. v, p. 209, 1908).

at these points, but it so happens that in one of the cases I have been able to do so and to obtain a result. At Burnt House I recently detected a low face of massive chalk about 6 feet behind the outbuildings (a shed with pigstyes at the back) shown on the map. In this face I was fortunate enough to find a specimen of *Belemnitella mucronata* and a small *Echinocorys*, which, although too much crushed for any certainty, appeared likely to be the var. *subconicus*. This evidence is sufficient justification for definitely identifying the *mucronata* zone at this point, which is about 150 feet from that zone as mapped. If the upper boundary of the *quadratus* zone be rectified to this extent (and it may well be that a more extensive rectification is due), it will at once be clear that only an improbable disturbance would carry the *quadratus* zone up to the Tertiary boundary just west of Burnt House, as is represented. The same rectification would presumably have to be made in the *Marsupites* plus *Uintacrinus* zone, and this would cancel the arbitrary expansion which has been given to the *coranguinum* zone at this point.

It therefore seems as if no reliance can be placed on the fluctuations of these zonal boundaries except where they are based on the

evidence of sections. They do not appear to be drawn with due attention to all available facts. They sweep across the deep valleys in the Upper Chalk indicated by the contour-lines at e.g. Burnt House or Standen Copse without any deviation. This could only be truthful in vertical chalk, and there is no ground for supposing that the chalk is vertical in these valleys though inclined in all sections. Even direct evidence from sections is liable to be ignored, as can be readily seen in the case of the great Downend pit. In Fig. 1 there is reproduced the rough outline of the pit. There is a high continuous face along the line *a b c*, while the line *d e* marks the strike as mapped for Dr. Rowe. If this strike is correct the highest chalk exposed in this face would be at the point *b*, and from *b* to *c* there would be a repetition of some of the beds exposed from *a* to *b*. This is, however, not the case. There is no repetition from *b* to *c*, but a gradual passage to higher chalk. The strike must therefore be at least as nearly E. and W. as the dotted line *f g*, and such a strike calls for zonal boundaries in this neighbourhood of a trend differing substantially from that given to them in the map and inconsistent with the thinning of the *mucronata* zone shown. (Incidentally I may say that close to the point *b* I have been able to identify the top bed of the upper band of *Offaster pilula* in the subzone of abundant *Offaster pilula* by its usual physical characters and large form of *O. pilula*. In downward succession from *c* to *a* there is first chalk without any marl seams. Below the first marl seam met with there are roughly 32 feet of chalk containing seven marl seams, and then come the two marl seams, 3 feet apart, with a flint line between them, which enclose this top bed.)

In the face of the foregoing considerations it is hardly possible to accept it as established that the *mucronata* zone has been entirely denuded away at any point in the Isle of Wight, and much of the alleged variation of that zone in thickness from point to point seems to rest upon a very insecure basis.

IV.—The Recent Geological History of the Baltic and Scandinavia and its importance in the Post-Tertiary History of Western Europe.

By Sir Henry H. Howorth, K.C.I.E., F.R.S., F.S.A., F.G.S.

Part I.

SOME years ago I was allowed to publish in the Geological Magazine[1] some papers on the recent geological history of the Baltic, in which I tried to bring before English readers the very important discoveries of the Northern geologists as affecting the general geology of the north-west of Europe and to extend their deductions. I was obliged to interrupt them for other work. Perhaps you will allow me to continue them some steps further, as we had reached a stage of some interest.

The northern portion of the Baltic, generally known as the Bothnian Gulf and comprising an area of over 1,877 square miles,

[1] For previous communications on this subject, see Geol. Mag., Dec. II, Vol. II, pp. 311, 337; Vol. III, pp. 1, 550.

is formed of two ovals separated by a narrow stricture between the towns of Umea in Sweden and Vasa in Russia, where the Archipelago of Quarken is situated. The more northern of these ovals, with an area of about 662 square miles, is known to the Swedes as Bottenviken, while the southern one, with about 1,215 square miles, is known to them as Bottenhavet. The former is now virtually a freshwater inlet. It contains but one living marine mollusc, and this only in its extreme southern part, namely, *Macoma solidula* = *Tellina Balthica*, which Nordquist reports from lat. 63·52 N.

As we have seen in previous papers, the latest raised beaches all round the coast of the Bothnian Gulf, including those at its head, prove that before the last changes of level took place in its shores there were four marine molluscs living there which have all now migrated further south, owing to the sweetening of the waters, caused largely by the inflow of the rivers having dominated that from the North Sea.

The reduction of the salinity may be measured by the fact that two of these molluscs, *Litorina litorea* and *L. rudis*, are both very adaptable littoral shells, seldom found at a greater depth than the low-water mark of spring tides, and often in large numbers in hollows of the rocks above the highest tides. Gwyn Jeffreys found the former living on the shore in a stream of perfectly fresh water during the recess of the tide (*Brit. Moll.*, ii, 106). The latter is often found in places overflowed by freshwater streams during the recess of the tide with its companions, the common mussel and the limpet (ibid., iii, 267). This means that they can live where the water is at one time fresh and at another salt, but not where, as in the Bothnian Gulf, the percentage of salt is always very small.

Both species occur in the lower raised beaches of the Baltic, and *Litorina rudis* has been found in them at Neder Kalix at the very head of the Bothnian Gulf. From them, as characteristic shells, these beaches have been called *Litorina* beaches, and the Baltic, at the time when they formed its margins, has been named the *Litorina* sea. In their strict sense the *Litorina* sea and the *Litorina* period came to an end when the uplifting of its bed and borders led to the shrinkage of the water from the area marked out by the *Litorina* beaches to its present contour. The change was limited to the restriction of its area and the re-arrangement of the range of distribution of its living contents, otherwise the *Litorina* sea had a continuous life with the present Baltic. This I have tried to show was not the result of a gradual change of level but of a spasmodic one.

The present conditions in regard to salinity and the present distribution of the mollusca, in the latitude of Bornholm represent very nearly what these elements were at the head of the Bothnian Gulf in the Litorina time.

As I pointed out in the second paper of this series, the *Litorina* sea was the successor of a great freshwater lake whose limits are marked out by the *upper* beaches of the present Baltic, and which contain no debris of marine life but only freshwater remains. From a notable shell the lake contained, it has been called the *Ancylus* lake or sea, which in turn gave its name to the *Ancylus* period. All this is now universally accepted and has been amply

proved by the researches of the Northern geologists, notably Schmidt, De Geer, Munthe, Holst, and Nathorst.

There is also a complete agreement among them that the cause of the conversion of the freshwater *Ancylus* lake into the *Litorina* sea was the breaking down of the land barrier which once united Southern Sweden with Pomerania and Mecklenburg on the one hand and Jutland on the other, and the opening of the three channels known as the Oresund, or Sound, and the Great and Little Belts by which the salt water of the North Sea first got access to the then enclosed freshwater Baltic, and converted it into a more or less brackish sea. This I have tried to show was not the result of the slow and gentle operation of current and normal denuding causes, but of a violent and sudden or very rapid dislocation of the earth's crust. It is necessary again to emphasize the difficulties of the opposite view, and the more so since I am constrained to believe that the dislocation was far greater, more wide-spread and important than has hitherto been thought.

The opposite view is really based on a professed adhesion to the theory of uniformity which is held to be inconsistent with catastrophe. Not uniformity in the sense that Nature working with the same tools and with the same potency and speed produces similar results, which is the keystone of modern science, but that Nature's operations at all times have been the same both in kind, potency, and rapidity as those which are working at this moment. This view, which still prevails with some geologists, is contradicted by all the evidence now available, notably by the gigantic and quite abnormal phenomena of the great mountain chains.

Let us, however, turn to our immediate problem and see what the evidence is.

First, we have the notable fact which, after a long discussion, seems to be now generally received, namely, that in Sweden, as in Britain and (as I shall point out presently) in Norway, there is no reliable evidence that the relative heights of land and water have altered in any appreciable way for a very long time, probably not for 2,000 years. This is also notably true of the Cattegat and the three waterways between it and the Baltic. In the case of the Cattegat, as in its gulf the Limfiord, the position of the kitchen middens in reference to the sea-level is an excellent test. In that great inlet which is girdled with these refuse heaps of primitive man, there is clear evidence that only slight changes have taken place in the relative position of the middens to the sea-level since the earliest stage of the Stone Age of Scandinavia. In the Sound and Belts the only notable changes have been those of silting up of estuaries and river channels, and of certain small signs of elevation (to which we shall refer presently). On the other hand, the position of the old maritime villages and towns, castles, and large trees on the sides of the waterways, both in Western Sweden and Denmark, are conclusive that there has been no upheaval or subsidence here for a long time, and no widening of the channels by denuding causes. In the case of the Sound we have a remarkable piece of evidence emphasizing this conclusion.

The island of Saltholm is planted in the very middle of the Channel. It is only raised a very few feet above the water, and is mentioned in the thirteenth century as a source of income to the Chapter of Roeskilde (see Geol. Proceedings, xi, 555), showing that there cannot have been much, if any, alteration there for many centuries. Nor, indeed (if we follow the teaching of rational uniformity), can we understand how, in a virtually tideless sea like the Baltic, the water could ever have had such potency as to bore through these channels.

In the present case we have no room for a draft on unlimited time, a favourite appeal of many geologists (who pursue deductive and not inductive methods), because, as has been pointed out by the Danish archæologists and geologists, the channels between the islands did not exist when the kitchen middens were laid down (see pt. iii of these papers, p. 12, etc.).

These arguments can be supplemented by others; thus, if the substitution of the *Litorina* sea for the *Ancylus* sea had been due to the gradual opening of the Baltic channels, we ought to have had a mixing and overlapping of the faunæ of the *Ancylus* and *Litorina* seas, which is not the case, but there is a complete gap between the two sets of deposits. On the other hand, it is virtually certain that the outpouring of fresh water from the Baltic, which, as we have seen, killed the oysters, *Tapes*, and other molluscs in the Cattegat, was not a gradual process. If it had been so we ought to have some evidence in the kitchen middens themselves, where the shells ought, under this maleficent influence, to have gradually become dwarfed and distorted, as they have elsewhere in similar circumstances, but of this there is no sign.

Again, if the process of boring these channels was due to the mere slow attrition by the waters on either side, how comes it that we find no kitchen middens at all on the shores of the three great channels, especially in their northern portions, nor yet in the smaller and subsidiary channels between the various islands. Surely all this is overwhelming evidence against the notion of gradual eating back of these channels by slow denuding forces, and the burden of proof of proving the contrary is very much indeed, thrust upon the advocates of the opposite view. I would add, as another positive argument in favour of the openings being the result of fracture, that in the case of the Sound the two sides which approach each other within 4,480 yards at Elsinore differ vastly in geological structure. On the Swedish side they are composed of Palæozoic rocks, and on that of Zealand of chalk, thus showing that the Sound forms a great line of fault where a rupture must sometime have occurred.

I must, therefore, take it for granted that the Baltic breach was caused by a tectonic movement of the earth's crust, and not by any slow denuding action. This tectonic movement has left, as I now believe, very notable evidences of its potency much beyond the narrow waters which intersect the Danish archipelago, and extending over a large part of the Chalk area of Southern Scandinavia and North Germany and its islands, and had the effect of completely shattering what were once continuous horizontal beds into their

present broken condition, and this, not in Tertiary times, but just at the threshold of the current geological period.

In order to set out the reasons for this important and really far-reaching induction I must be allowed to make some preliminary remarks on what may seem to be unnecessary to those who have not measured, as I have done, the *vis inertiæ* of the older type of geological mind which still survives among certain veterans of the science.

There was a time when the various movements of the earth's crust (of which the evidence is patent enough) were attributed to the operations of some unknown but postulated subterranean energy which was supposed to be able (in limited areas) to lift up by forces acting perpendicularly or to let down by similar impulses great masses of the earth's strata, and thus to largely cause the diversified features of the earth's crust. This view is now only held by a small and shrinking body of geologists. The great mass of them who have surveyed geological problems on a continental scale and realized how far-reaching the phenomena must in some cases have been, have found it impossible to maintain an hypothesis which will not meet the facts as we now know them. The more influential of the modern teachers of the science are no longer satisfied, like the extreme glacialists are, to rely upon causes for which no adequate physical justification has ever been produced, and which necessitate the postulating of qualities and characteristics in the materials which build up the earth's solid envelope which refuse to be verified by experiment. These newer men who recognize that geology must in the long run rely upon physics to supply it with a workable platform have found a perfectly satisfactory reason for earth movements on a very big scale and over very large areas. The postulate they stand upon in this matter is, that the earth is inevitably losing its heat by radiation and in the process is shrinking, and in shrinking it has to compel its upper strata to accommodate themselves to a smaller space. The result is great lateral (and not perpendicular) thrusts which have squeezed the beds into wave-like and sinuous curves and ribbons, with alternating anticlinal and synclinal folds.

On this point let me quote two excellent authorities, and as there must be no quarrel about their meaning I will quote their actual words:—

Suess says definitely: "Es giebt keinerlei verticale Bewegungen des Festen, mit Ausnahme jener, welche etwa mittelbar aus der Falten bildung hervorgehen. Die Felsarten der Erde besitzen in keinerlei Gestalt jene räthselhafte elevatorische kraft welche man ihnen in einer Zeit Inzuschreiben geneigt, und vielleicht bis zu einem gewissen Grade berschtigt war, im welcher. . . . Wir werden uns enschliessen müssen letzte Form der Erhebungstheorie die Doctrin von den Saecularen Schwankungen den Continent zu verlassen." Heim urged the same view (Jahrbuch k.k. Geol. Reichsanst Wien, 1880, p. 180).

Lapparent is still more positive. He says: "Il ne s'agit pas d'avantage d'opposer à la doctrine des soulèvements absolus produits par des forces qui agiraient directement de bas en haut, une protestation devenue sans objet. Car les partisans des impulsions

verticales sont, de nos jours plus, que clair semés et en dehors de quelques rares attardes, personne n'oserent encore attribuer à une telle action une part sérieux dans la formations des montagnes" (Bull. Soc. Géol. France, ser. III, t. xv, p. 217).

Again, the same graphic and lucid writer, speaking of what he calls "impulsions verticales", says: "Ils s'expliquent sans difficulté si on les considére comme la production des movements généraux d'une écorce soumise à des efforts latéraux de compression, développées par la necessité où elle se trouve de se plier aux changements de dimension du noyau interne. De cette manière certaines parties se gonflent l'océan, tandis que d'autres semblent l'attirer dans les sillons qui vont se creusant de plus en plus" (*Traité de Géologie*).

It is assuredly by this process that the tectonic changes in the earth which have diversified its surface into mountain and valley have been in the main induced. It is equally plain that this process of bending into undulations and curves cannot go on for ever with such very tough materials as the earth's crust is largely formed of, without a break. The tension must presently be so great that the rocks will give way and split, and form huge rifts and crevasses and raw scarps and cliffs. Scandinavia presents us with most admirable evidence of the results of this crumbling process on a large scale in producing the diversified features of the country: a great many of us are witness to that. They meet us at every turn in the contortions and folds made violently, and involving great breakages and gaping wounds in the hardest crystalline rocks as well as in those of Secondary age. The Alps, the Pyrenees, and the Himalayas give us similar very notable samples from Tertiary times, of raw angular tears and rifts, gullies and nullahs, and perpendicular scarps and faults, as well as huge anticlinal and synclinal bends and overthrows. Mohn has picturesquely described the results in Norway. "The oldest formations in Norway," he says, "are greatly bent, compressed, and distorted, and their parts forcibly dislocated, alike as regards situation and relative height. Formations that in the interior lie at a height of several thousand feet are on the coast found level with the surface of the sea; strata resting on the summits bordering a lake or the shores of a fjord are again seen on islands in such lakes or fjords and level with the surface of the latter. One side of a valley exhibits a profile which, in regard to the height of the various strata, differs materially from the profile of the opposite side. The whole rocky shore is cut up in various directions, and the several laminæ are now sunk beneath, now raised above, those adjoining them. These dislocations have been caused by fissures, which in many places can be pointed out, and the number of such recorded faults of dislocation increases almost every year. The direction of the fissures is manifestly of the greatest assistance in indicating the form exhibited by the surface of the country. The subsidence between two fissures produces a valley or fjord; its rise, on the other hand, a height or a promontory. Professor Kjerulf has succeeded in showing that the entire system embracing the valleys and fjords of Southern Norway may be easily referred to four principal directions

round about the principal directions of the valleys and fjords, and are found grouped with predominant frequency."

What Mohn says of Norway has been equally well said by De Geer about the tectonic structure of Sweden. "Up to this time" (i.e. 1893), he says, "I have levelled the marine limit at about seventy different points on the southern and central parts of Sweden and in a few places in Southern Norway. For Northern Sweden I have three or four approximate but important determinations by Högbom, Svevonius, and Munthe. . . . All the observations relate to one system of upheaval with the maximum uplift in the central part of the Scandinavian peninsula along a line east of the watershed. . . . Here the land must have been upheaved somewhat more than 1,000 feet (more than 300 metres), and around this centre the isobars are grouped in concentric circles, showing a tolerably regular decrease of height in every direction towards the peripheral parts of the region, until the line for zero is reached, outside of which no sign whatever of upheaval is to be found" (Bull. Amer. Geol. Soc., 1892).

Elsewhere, De Geer, who has done so much for explaining the internal structure of the great Swedish anticlinal, has carefully co-ordinated the facts and drawn lines of isobars showing that they point to a focus of elevation along the medial line of the uplift, curving down to lesser heights of similar altitude and *synchronous in date on the eastern and western sides of Sweden respectively* (see D. G., *Over Scandinaviens Nivaförandringar under Quartarperioden*, p. 56). He tells us the first points he determined were in Scania, and the heights of the different points were nearly equal on both sides of the axis; some were 50 metres high, somewhat more towards the south; adding that he afterwards obtained these successively at 48, 42, 37, 32, and 21 metres, and that in quite open localities.

Such being the structure of the great Swedish anticlinal, is it strange or unexpected that it should in the south pass into a corresponding and complementary synclinal hollow, with evidences, not of rising, but of sinking? These are present (as I showed by much evidence in the fourth part of this series of papers) all over the South Baltic and extending to the North German coast. The line of greatest depression, known as Forchhammer's line, runs east and west through the middle of the Southern Baltic. Would it not be strange if the lifting up of this long whale-backed peninsula and this corresponding synclinal movement in the south had taken place without any breaks and breaches at the points of greatest stress, namely, where the upheaval and the subsidence, caused by the lateral thrust, were the greatest? It would indeed be strange if it were not so. In the subsiding south, as we shall see, the material was chalk; in the north, as we shall also see, the uplifted Primary rocks were the ones to give way and be broken, and in both cases presenting the clearest evidence of violent or momentary dislocations in places on a great scale with tremendous breakages in the rocks. The great subsidence in the Southern Baltic is partially attested by submerged forests and peat bogs south of Scania. If the submergence had been gradual and progressive along a disappearing beach, these fragile relics would

have been long ago destroyed, and, like similar remains elsewhere, they attest a sudden submergence.

I mention all this to establish a prima facie case. Let us now turn to more direct evidence as displayed in the Danish islands and on the South Baltic coast. The greater part of the Danish islands are covered with drift beds in many places of enormous thickness. These largely hide the subjacent chalk. It is not so covered everywhere, however. In the great Danish island of Zealand and in its small satellite Moen the chalk is in part exposed, and in both we have some valuable evidence for our purpose. In the former the part of the chalk that is visible is in a great measure undisturbed, but not everywhere.

Rördam tells us the chalk of Zealand is for the most part covered with diluvian beds sometimes 60 metres thick and enclosing large masses of chalk. He describes a section thus: "On voyait la Craie recouverte d'argile morainique jaune-rouge, contenant aussi de grandes portions de masses de Craie triturée et petrée d'argile et de pierre. La figure 1, p. 8, fait voir incrusté dans l'argile morainique un assez grand bloc de Craie de forme irreguliere" (n.ed., vi, 128).

Let us now turn to the small and geologically celebrated island of Moen, separated by a narrow passage from Zealand, the land on both sides of which channel consists of chalk. The facts there were long ago carefully collected on the spot and their inevitable lesson pointed out by two excellent witnesses, namely, Lyell and Forchhammer. Lyell, writing in 1873, speaks of the phenomena presented by the island as being of a class which were thought by the earlier geologists to belong exclusively to epochs anterior to the existing fauna and flora, and quotes as examples faults and violent local dislocations of the rocks and sharp bendings and foldings of the strata, which we so often behold in mountain chains, and sometimes in low countries, especially where the rock formations are of ancient dates. He then proceeds to quote the island of Moen as a striking illustration of such convulsions, to which he assigns a post-Glacial or Pleistocene date. He describes it as about 60 miles in circumference and as consisting of white chalk several hundred feet thick, overlaid by boulder-clay and sand made up of several divisions, some stratified and some unstratified, the whole having a mean thickness of 60 feet, but being sometimes twice that thickness, and containing in one of its oldest members fossil marine shells of existing species. He goes on to say, "Throughout the greatest part of the island the strata of the drift are undisturbed and horizontal, as are those of the adjacent chalk, but on the north-eastern coast they have been through a certain area, bent, folded, and shifted, *together with the beds of the underlying Cretaceous formation.*" "Within this area they have," he says, "been even more deranged than in the English chalk-with-flints along the central axis of the Isle of Wight in Hampshire, or at Purbeck in Dorsetshire. The whole displacement of the chalk is evidently posterior in date to the origin of the drift since the beds of the latter are horizontal or inclined, curved, or vertical where the chalk displays signs of similar derangement."

Again he continues, "Although I had come to these conclusions respecting the structure of Moen in 1835 after devoting several days in company with Dr. Forchhammer to its examination, I should have hesitated to quote the spot as exemplifying convulsions on so grand a scale of such extremely moderate date, had not the island been since thoroughly investigated by a most able and reliable authority, the Danish geologist, Professor Puggaard, who has published a series of detailed sections of the cliff." Commenting on one of the sections showing great contortions, Lyell says, "Where the cliff is 180 feet high there is a sharp flexion shared equally by the chalk and the incumbent drift. In each we observed a great fracture in the rocks, with synclinal and anticlinal folds exhibiting in cliffs 300 feet high, drift beds participating in all the bendings of the chalk."

Near the northern end of Moen's Klint, at a place called Taler, more than 300 feet high, are seen similar folds so sharp that there is an appearance of four distinct alternations of the Glacial and Cretaceous formations in vertical or highly inclined beds, the chalk at one part bending over so that the position of all the beds is reversed. But the most wonderful shiftings and faultings of the beds are observable in the Dronningestol, part of the same cliff, 400 feet in vertical height, where the drift is thoroughly entangled, and raised up with the dislocated chalk. Lyell comments on these facts and says, "It is impossible to behold such effects of reiterated local movements, all of post-Tertiary date, without reflecting that but for the accidental presence of the stratified drift, all of which might easily (when there has been so much denudation) have been missing, even if it had ever existed, we might have referred the verticality and flexures and faults of the rocks to an ancient period, such as the era between the Chalk with flints and the Maestricht Chalk, or to the time of the latter formation or to the Eocene or Miocene or older Pliocene eras" (Lyell, *Antiquity of Man*, 4th ed., pp. 388–98). Not the least wonderful of these dislocations is the height to which the chalk was thrown up in some of the cliff sections. Those who are familiar with the similar phenomena in Norfolk, which I have known well and commented on for many years, will see how in every detail they repeat those of Moen as here described by Lyell and which I have no doubt were caused in precisely the same way and at the same time.

It seems very probable, says Reclus, that having subsided Moen was again raised above the waters. It is really composed of seven distinct islets whose intervening channels have since been filled up. In 1100 A.D. it still formed a group of three, and Borre (now lost among the fens) stood on the beach in 1510, when a Lubeck fleet anchored in front of the houses and burnt the place to the ground.

Moen is a natural step to the island of Rugen and the southern coast of the Baltic. Here we meet with precisely the same kind of chalk beds covered with drift, and torn and dislocated in the same way and clearly at the same time. Reclus, in describing them, speaks of "the rocky shores of Moen and the lofty headlands of Rugen formerly united but now separated by a strait 33 miles broad and 12 fathoms deep". Rugen, again, has its counterpart in the Baltic coastlands near Stettin.

Neumayr (in his *Erdgeschichte*, ii, 586–7), describing the Chalk beds at Rugen and Stettin, speaks of the way in which diluvial soft beds occur in masses detached from their matrix and transported elsewhere; and of their being occluded in beds of the same age but of different composition, with their internal laminæ undisturbed, just as we find them in Eastern England. In other places the strata are largely crushed and squeezed and tossed about or faulted. "Especially notable," he says, "is the phenomenon which occurs in the case of the deposits of white chalk on the shores of the Baltic as at Rugen near Stettin and in other places of North Germany; huge masses of chalk have been here detached and planted in the midst of the boulder-clay and have caused great disturbances in it. Thus Remele found in the boulder formation at Stettin a huge slab (*schallen*) of chalk almost 2 kilometres long, that is more than a mile long, and of the thickness of 25 metres, embedded in the drift beds. He also came across similar instances elsewhere." Neumayr speaks of the great cliffs of chalk facing the sea at Rugen and much broken. These breakages have only taken place in certain places; while in others the strata lie in their old horizontal position. Frequently there may be seen great masses of chalk built up out of a congeries of confused chalk lumps, while in many places the chalk masses rest on diluvial sand clay and boulder-clay, or these latter have forced themselves between the boulders of chalk.

The latest authority on the Chalk of Northern Germany is von Linstov of Berlin, whose paper entitled "Die Techtonik der Kreide in Untergrunde von Stettin, etc." contains some valuable materials for the elucidation of the problem, since they consist largely of borings and testings of the different exposures. In none of these borings has the chalk in situ been pierced; but in several cases great masses of chalk proved to be true boulders lying in the Drift like those of Norfolk. This was the case with the famous great chalk boulder found at Frickenwalde in the middle of the last century, and pronounced by Deecke in his *Geology of Pomerania* to be 2 kilometres long and 34·41 metres thick. It was found to be underlaid by boulder clay, and what he calls glacio-fluviatile beds. So with the great mass of chalk found at Katharinenhof, which was of great size, thickness, and weight. A similar pair of great masses was described by C. Muller, one from Sparrenfelde, west of Stettin, and the other from the exercise ground at Kreckof. These were found when bored in 1898 to give the same result, namely, they proved to be portentous boulders.

Linstov (op. cit., 144) also gives profiles of numerous faults occurring in the chalk of the same district. He discusses the date when the great breakage occurred, and rejects as impossible the notion that they were pre-Glacial, and like Credner, in Rugen, he puts them between the so-called first glaciation and the second one, that is, makes them post-Pliocene.

The following table gives the details of other great masses of rock occurring as boulders in the drift of chalk and Tertiery strata from North Germany, which have been similarly tested:—

Geological Borings in Stettin District.

		Metres.
Finkenwalde.	Boulder of so-called chalk.	over 35 long
Gollnof, north-east of Stettin.	Septaria clay.	about 100
Züllchof.	Boulder of Septaria clay.	2–36·5
Gartz.	Boulder of Septaria clay.	10–41
Stralsund.	Boulder of chalk.	100
Gollenberg, near Köslin.	Miocene and Oligocene.	about 100
East Diervinof.	Senonian chalk.	10–27
Treptov on the Rega.	Boulder of Senonian Chalk.	31
Fort Chernoy, near Sonnenberg (Neumark).	Miocene.	65–8
Steinitten in Samland.	Under Oligocene, Miocene, and Senonian.	7–20
Osterode in East Prussia.	Miocene, Oligocene, and Senonian.	34
Frankfort on the Oder.	Miocene.	4–80

According to Credner the drift beds overlying the broken and dislocated chalk of Rugen are divisible into two sets. One of them, the lower one, conformable with the chalk and which consists of two greyish-blue boulder-clays, separated by bedded sands, and which follow the fortunes of the chalk, and the other of certain boulder clays, gravels, and sands which overlie the chalk unconformably and disregard its contortions. It is possible that the latter may be part of a secondary movement, which has since the great submergence in the Southern Baltic reversed the process to a small extent and left its traces in different places by certain later breaches at low levels.

I now propose to say a few words about the exact parallel we have in Britain to these Danish and German Chalk dislocations in the disrupted chalk of Norfolk and elsewhere in England. The phenomena are precisely the same in detail and belong, so far as we can judge, exactly to the same period, and were due to the same cause. In England they have been made the pet toys of the Ultra-glacialists, especially of those of them who were entrusted with surveying the surface beds of Eastern England, and who quite ignored these northern parallels which have been so carefully examined and exploited by German geologists with great ardour of recent years. I may, perhaps, be permitted to recall some of the arguments I adduced long ago against the notion that the dislocations in the chalk of Norfolk and the disposition of their broken debris were the handiwork of a hypothetical ice-sheet or of ice in any form, and were really the results of tectonic rupture of the chalk. The proposed and quite imaginary ice-sheet, the mode of production of which is admittedly an unsolved riddle, is supposed to have crossed the North Sea from Norway when that country must have been at a much lower level, as I propose to show later, to have crossed the vast submerged valley (then much deeper) which bounds Norway on the west, and to have travelled hundreds of miles without any adequate thrusting force from behind it: when therefore, if it moved at all, it must have been by a necessarily very slow progression of its layers over each other in the fashion of a plastic body with very little plasticity, and with a vertically extinguished motion at its base. In order to secure this, it must have been piled up to a portentous

height; all this is jauntily suggested, forgetful of the fact that the *modulus* of ice is such that it crushes and liquefies when subjected to a very moderate pressure. This fantastic machine is then supposed to have climbed up into Norfolk, and when only moving at the speed of an exhausted tortoise to have done two kinds of dynamical work quite inconsistent with each other, and at the same time, namely, passed over numerous beds of stratified crag sands without disturbing their layers, and at the same time to have completely broken up the solid chalk, causing the greatest confusion in its beds, which are crossed by endless faults, some of them scores of feet in extent. It then proceeded to detach huge cakes of solid chalk hundreds of yards long from the matrix (by what mechanical process has never been explained), and in the process to have shattered the great mass of the upper layers into myriads of unweathered angular lumps and boulders and thousands of tons of chalk dust, and to have bodily lifted up and carried in its terrific and destructive arms, not only the great cakes and ribbons of chalk just named, but also huge masses as big as houses, and whirled them along, and then deposited them in the midst of stratified and beautifully laminated sands without causing any breaks either in the lines or the curves of the layers, which it arranged in concentric form about the intruded masses, while in other places it laid down these sands in huge curves with re-entering curvatures without any breaks in the lines, and in other places to have torn up masses of these sands with their internal structure undisturbed, and then carried off these fragile lumps unbroken and uninjured at the time when it was pounding and smashing and tearing the chalk to the depth of scores of yards. All this and much more I have set out years ago in papers in the Geological Magazine, notably in a discussion of the "Dislocations in the Chalk of Norfolk" in the volume for 1907.

Since, then, it has become plainer every day that these dislocations were not confined to Norfolk and Suffolk but were synchronous with great movements of the chalk south of the Thames *entirely out of reach of any ice-sheets*; in the border of the English Channel, in Hampshire, the Isle of Wight, in Northern France, in Flanders, and elsewhere. The evidence seems to point to the same impetus having been the real cause of a great deal of the shaping of the north and south Downs and of the synclinals which are correlated with these whale-backed ridges, which in places have pot-holes on their surface containing casts of Miocene shells, showing how late the upheaval must have been. The same movement doubtless threw down the chalk in Holland to a portentous depth, carrying with it in places hundreds of feet of rearranged Crag and Pleistocene sands. It is clear that the same portentous cause must also have operated in the Baltic lands, inducing there a repetition of precisely the phenomena we have in Norfolk, and, so far as the evidence leads, quite contemporaneously. All this capacity and work has been attributed by a long dominant school especially potent in the arcana of Official geology to the handiwork of ice, whose proved impotence to compass such work they have entirely ignored, and who have refused to listen to those who had been trained in the more precise methods of

physics, and warned them over and over again of the blind alley they were following. They were faithfully copied for a long time by the geologists of Germany, who dealt similarly with their domestic problem of explaining the shattered chalk of their own country. Among them, the most notable champions of ice were Wahnschaffe and Scholz, and more lately Philippi. The first notable geologist in Germany to make an effective revolt against the once current explanation of these Cretaceous masses and their movements was Von Koenen, who attributed the gigantic dislocations and movements involved to tectonic earth movements on a great scale. He was presently supported by Berendt, Hermann Credner, Cohen, Deecke, R. Credner, and lastly by O. Jaekel and K. Keilhack. They were also at one in regard to the date of the dislocations and the accompanying phenomena, which they attributed to post-Tertiary times.

These German explorers, especially the later ones, have sifted the evidence with skill and pains, and have tested some of the initial difficulties with the boring rod. They are agreed that the phenomena are not explainable by the action of ice, a view in which my friends Professor Bonney and the Rev. E. Hill completely concur. After paying two visits to Rugen they affirm that no evidence can be found there to support the ice theory. "We shall be ready," they say, "to admit the potency of ice-sheets as excavators, and benders or breakers of rock masses when any evidence worthy of the name can be produced in proof that they operate in these ways; but though we have diligently sought for it in the field we can only find it asserted on paper" (Quart. Journ. Geol. Soc., lvii, pp. 16, 17). This is precisely the conclusion I have maintained in regard to the English beds for many years.

Lyell and Credner both agree that the drift beds which lie conformably to the Chalk in the Baltic lands and follow its convolutions and lines of fracture were deposited before the great disturbance took place, and Lyell distinctly describes it as post-Glacial, that is, as I prefer to say, posterior to the deposition of the drift, and it is a notable fact that that great master, who saw a good deal further than some of his professed disciples, should have so emphatically adhered to the view which seems the only view consistent with the doctrine of uniformity that periods of great disturbance in the earth's crust were not confined to older geological periods, but have occurred as late as Pleistocene times. Those who were reproved by Lyell so forcibly seem to forget that it is in the very earliest geological periods that we have the fewest evidences of contemporary dislocations on a great scale, and also the largest extent of still undisturbed strata, while all the greatest dislocations known to us took place in later periods, notably in Tertiary times, as the Alps, which have been lifted up 21,000 feet since Eocene days, and the western Himalayas quite as high since Pliocene times, bear witness. Is there any reason under heaven why the process should have stopped in Tertiary times? How can anyone who believes in rational uniformity maintain such a theory?

Have not (as the Scandinavian geologists I have quoted have shown) the two gigantic peninsulas of Greenland and Scandinavia

been bodily uplifted from far below sea-level to a height of many hundreds of feet in post-Tertiary times, as attested by the shell beaches that girdle their flanks, and when the present living mollusca were tenanting the present seas that wash their shores? Is it a rational induction or the reverse to argue that if such enormous movements in such tough rocks were the result of lateral thrusts caused by shrinkage of the earth's crust, which both induction and experiment combine to establish, that thrusts on this scale could hardly occur without the most serious breakages and fractures?

I have very little doubt, therefore, that the chalk dislocations of the Baltic were consequential on the uplift of the great hog-backed Scandinavian peninsula, and were almost certainly synchronous with the opening of the Baltic breach, which certainly dates from the human period.

(To be continued.)

V.—The Genesis of Tungsten Ores.

By R. H. Rastall, M.A., F.G.S.

(Concluded from the July Number, p. 296.)

Part IV: Secondary Tungsten Deposits.

At the present time a large proportion of the world's supply of tungsten ores comes from secondary (detrital) deposits of various kinds, formed by the normal denudation and redeposition of primary ores exposed at the surface to the agents of weathering and transport. It is impossible to form any idea of what fraction of the world's output actually comes from these sources, since the published statistics do not draw any distinctions in this respect, but the amount is undoubtedly large. Although of such great economic value, the secondary deposits do not show any features of special interest, and a lengthy description is unnecessary.

From this point of view the outstanding feature of the tungsten minerals is their great stability and resistance to any kind of chemical or mechanical alteration. Hence, like cassiterite and gold, they are particularly prone to occur in both residual and alluvial deposits. In many of the published descriptions, and especially in the technical journals, a good deal of confusion is found to exist between the residual deposits, where the material is still more or less in place, and the true alluvial or transported deposits. From their very stable character it follows that tungsten minerals must tend to remain unaltered in the gossan of lodes and other masses and also to concentrate in the shoad. Hence a kind of secondary enrichment is found on the weathered outcrops of lodes. This is, of course, not really an enrichment in tungsten ores, but rather a removal of other constituents of a less stable nature, leading to a concentration of the more resistant minerals of the weathered mass. From their stability and high density it also follows that the tungsten minerals are specially liable to occur as placers and other forms of transported deposits. The same properties also lead to a natural concentration in such deposits, especially in the lowermost layers, resting on the bed-rock, and in natural riffles. In this

respect both wolfram and scheelite behave like stream-tin, gold, and platinum. In fact, the properties of wolfram are so like those of cassiterite that their separation by mechanical processes is very difficult, and it was not till the introduction of magnetic separation that this difficulty was overcome.

Geologically the secondary tungsten deposits are so simple and straightforward that it seems unnecessary to describe any individual examples, while the practical details of their exploitation, concentration, and after-treatment do not fall within the scope of this paper. Essentially they consist mainly of breccias, gravels, and sands, formed to a large extent by water-action, and occasionally resulting from the effect of dry denudation in regions of small rainfall. The briefest possible reference may also be made to the "Head" of Bodmin Moor and other parts of Cornwall, so admirably described by Mr. Barrow.[1] In this connexion it may also be mentioned that of late years it has been found profitable in many instances to work over the old tin-dumps for wolfram, which was thrown away as worthless by the earlier miners.

Part V: Conclusions.

In the foregoing pages an attempt has been made to give a general account of the mode of occurrence and mineral paragenesis of the tungsten ores. Attention has been paid chiefly to the theoretical side of the subject, with a view to elucidating as far as is possible the genesis of the ores and their relation to the associated minerals and rocks.

Taking first the wolframite deposits, it is found that these occur most commonly along with cassiterite; other minerals also accompanying them in nearly all cases are arsenopyrite and molybdenite. The gangue minerals nearly always include some that are characteristic of the pegmatitic or pneumatolytic dykes or veins. Furthermore, it appears that this general association of tungsten, tin, molybdenum, and arsenic may be further subdivided on the basis of the rarer metallic elements present into subtypes or local metallogenetic provinces, such as the uranium province of Cornwall, the tantalum-mobium provinces of Burma and Dakota, and so forth. Another major subdivision is afforded by the tungsten-tin-silver-germanium group of Bolivia. From the evidence brought forward it may be regarded as established that the tungsten-tin deposits are derived in all cases from granitic magmas. Wolfram and cassiterite are found as original minerals in granite, being direct products of the crystallization of the magma; as constituents of pegmatite dykes within and in direct continuity with the granite, while wolfram is also found in quartz veins, so-called, which are continuations in space of pegmatite dykes. Hence there is no real distinction between a pegmatite dyke and a quartz vein, and the study of these lodes lends strong support to the idea that many of the larger quartz veins are in fact formed by crystallization of the last residues of an acid magma. The separation of these residues is, of course,

[1] Quart. Journ. Geol. Soc., vol. lxiv, p. 384, 1908.

essentially a process of differentiation, which may be described as fractional crystallization. Processes of this kind are generally described as pneumatolytic, but there does not seem to be any need for the use of the word in this connexion, since as commonly understood it seems to imply something unusual and out of the common order of events. This kind of differentiation is perfectly normal; its final results depend mainly on the extent to which highly volatile compounds and water were present in the original magma. The more of these are present the lower will be the freezing-point of the final product. The effect of alkaline tungstates in lowering the freezing-point of acid silicate melts, and thus enabling quartz and acid felspars to crystallize, has long been known and used in petrological and mineralogical research. It appears that during the freezing of the residual magma cassiterite is formed first, then comes wolframite, while last of all quartz and fluorite are formed, resulting in veins of quartz and fluorite without metallic minerals, or even of fluorite alone.

The evidence afforded by the wolframite lodes without tinstone is entirely in conformity with the foregoing considerations. There is a continuous gradation between the two types by diminution of tinstone; the series passes through the wolfram-quartz lodes, in which molybdenum and arsenic tend to disappear, whereas other sulphides tend to increase. As the final member of the series we have the association of wolfram with siliceous gold ores, as in Colorado, and of scheelite with gold ores in California. This relationship of tungsten to siliceous gold ores is of much interest in connexion with the classification of ore-deposits proposed by Spurr.[1] This author regards all ore-deposits as due to differentiation of igneous magmas, the variations depending on the amount of water and rarer elements originally present in the magma. According to this scheme, the bulk of the tungsten deposits belong to the first or pegmatitic type, which is characterized by tin, tungsten, molybdenum, with tourmaline and topaz as gangue minerals. Some of the tungsten ores, however, both wolfram and scheelite, extend into the second group, "the free-gold pyrite zone with quartz."

Spurr correlates these differences mainly with depth, since he regards all ores as deposited by ascending solutions. Although this idea is highly probable, it cannot yet be regarded as demonstrated that the tin-tungsten lodes were formed at a greater depth from the surface than the gold lodes or the cupriferous pyrite deposits. This point, however, is not of very great importance. It is a fact, however, that the occurrence of tungsten ores when studied in detail lends strong support to the idea of a definite sequence of differentiation of the rarer metallic contents of the granitic magmas.

The tungsten ores are definitely oxidic in character, and in many ways afford a strong contrast to the behaviour of the sulphidic ores. They appear to belong characteristically to acid magmas, whereas most of the great masses of sulphides are connected with basic intrusions. There is also a strong contrast between the two classes

[1] Spurr, *Economic Geology*, vol. ii, p. 781, 1907.

in the elements which act as "carriers". In the case of the tungsten ores the most important of these is undoubtedly fluorine; boron is a very common associate, but it is not known whether it plays any actual part in the formation of wolframite or scheelite. A detailed study of the literature does not lend any support to the statement sometimes found in textbooks that apatite is a common associate of this group. In fact, minerals containing phosphorus or chlorine seem to be conspicuously absent.

Another point of interest is that the tungsten minerals are all characterized by a high degree of stability and consequent resistance to chemical action of any sort. Hence they do not undergo any alteration in the zone of oxidation and are not carried down in solution into the zone of cementation. This is equivalent to saying that they do not undergo secondary enrichment. It follows from this that tungsten lodes are always primary in the narrowest sense of the term, and this is a fact which should be taken into consideration in the valuation of such deposits. The behaviour of tungsten lodes in this respect is on the whole very similar to that of the tin lodes, and shows no resemblance to the characters of the copper lodes, for example.

To sum up, it may be said that the tungsten ore-deposits are of magmatic origin, being formed by the natural and normal concentration in certain fractions of the magma of a group of constituents, metallic and otherwise, which tend to occur together owing to the similarity of their chemical and physical properties under the conditions that prevail during the later stages of magmatic consolidation. Of these constituents the most important are tungsten, tin, molybdenum, arsenic, fluorine, and boron. These constitute the general paragenesis, while certain regional subtypes can be distinguished, characterized by uranium, mobium, tantalum, and possibly others not yet specifically distinguished. In certain cases transitions can be traced to other groups of ore-deposits containing gold, silver, copper, zinc, and lead. Some of these mixed deposits may be explainable by deposition of metals in the same locality at two or more distinct periods, while in other instances the differentiation of the original magma may have been incomplete and ill-defined. At any rate, it is clear that in the typical cases the prime factor at work has been differentiation of igneous magmas, and the study of the genesis of the tungsten ores lends the strongest support to modern views as to the origin of ore-deposits in general. It is now becoming increasingly manifest that the study of the characters and origin of the metalliferous rocks is an extraordinarily interesting and important branch of the science of petrology, and that in the past this branch has been unduly neglected by workers on the purely scientific and theoretical side. A detailed investigation of the chemical and physical laws governing the formation of the oxides and sulphides of the igneous rocks, on the same lines as already applied by many workers to the silicates, could not fail to yield results of the highest scientific interest and of the utmost practical value.

REVIEWS.

I.—The Geology of the Barberton Gold-mining District. By A. L. Hall. Memoir No. 9 of the Geological Survey of the Union of South Africa. pp. 347, with 58 plates, 40 text-figures, and a coloured map. Pretoria, 1918. Price 7*s.* 6*d.*

IN this comprehensive memoir Mr. Hall gives a detailed account of the physiography and geology of the important Barberton gold-mining district in the Eastern Transvaal and Swaziland. The physical characters of the region present many features of interest: in the west comes the great Drakensberg escarpment, here consisting mainly of strata of the Potchefstroom or Transvaal system. The "Barberton Mountain Land", consisting of a large number of mountain ranges, is composed chiefly of the slate-quartzite group of the Moodies Series, while in the south is a great granite plateau.

The area shows a typical development of the Swaziland system, which is subdivided into three groups, the Onverwacht Volcanic Series, the Moodies Series, and the Jamestown Series: each of these is penetrated by the older granite, while the Transvaal system rests on all of them with a major unconformity. It is shown conclusively in this memoir that the granite is of later date than the Swaziland rocks, and the chief interest of this point lies in the fact that the gold reefs are mainly to be found in the metamorphic aureole of this granite. The Karroo System covers only a small area, including the Komatipoort coal-field. The Moodies Series consists of sedimentary rocks, chiefly slates and quartzites, whilst the Jamestown Series now takes the form of chloritic and talcose schists and other types probably derived from basic igneous rocks, rather like the Keweenawan of North America. The later basic intrusions are of little or no economic importance.

Besides the gold-fields of the De Kaap area and Northern Swaziland, the district also possesses economic possibilities in the form of extensive deposits of magnesite and talc, while cassiterite has also been worked on a small scale in one or two localities.

R. H. R.

II.—Report of the Mines Branch of the Department of Mines, Canada, for the year 1916. pp. 183, with 14 plates and 10 figures. Ottawa, 1917. Price 25 cents.

THIS report contains a general summary of the work of the Department for the year, together with individual reports on various subjects that have been specially investigated. Among these are notes on occurrences of iron-ores and building and ornamental stones, and on a reconnaissance for phosphate in the Rocky Mountains and for graphite in British Columbia. In Canada, as in England and in the United States, much attention is now being paid to sands suitable for metallurgical purposes, and a large amount of work has been carried out by the Department on this subject. Attention was also paid to the possibility of removing lime from impure magnesite, such as is found in the Grenville district; this is

contaminated with dolomite, and when the mixed rock is heated to about 1,000° C. in an electric furnace the magnesite becomes darker in colour. On slaking the magnesite material forms a very coarse gritty powder, while the dolomite lime forms a milky paste which can be easily removed by washing; hence a separation can be readily effected. The fuel-testing and ore-dressing sections of the Department have also conducted much useful work in various special directions, and the whole report gives evidence of much energy and activity.

R. H. R.

III.—Geology and Ore-deposits of the Bawdwin Mines, Burma. By J. Coggin Brown. Rec. Geol. Surv. India, vol. xlviii, pt. iii, pp. 121–80, with 8 plates, 1917.

THE Bawdwin ore-deposits are enclosed in a series of rhyolites and rhyolitic tuffs, which form a kind of dome protruding through the younger Pangyung (Cambrian or Ordovician) strata. Above these is a regular sequence to the Devonian, and then an unconformable series of Jurassic sandstones, clays, and limestones. At one horizon in the Silurian many specimens of *Monograptus* have been found. The rocks of the volcanic series are much weathered, and their original character is not always easy to determine, but all the ore-bodies seem to lie in the tuffs rather than in the rhyolites. The ore-bodies all lie in a well-marked zone or channel about 8,000 feet long and 400 or 500 feet wide, possibly connected with an ancient fault system.

There are three distinct lodes of lead-silver-zinc ore, but of much more importance is the Chinaman ore-body, which is an enormous replacement deposit of zinc-lead-silver ore, lying on the hanging-wall side of the ore-channel. The essential constituents of the ores are galena and zinc-blende with a little pyrite and chalcopyrite. The ore is always argentiferous, showing on the average about 19 oz. to the ton. The gangue consists of metamorphosed country rock and a little quartz. The zinc ore appears to be as a rule of earlier formation than the galena, while the copper and iron sulphides are intermediate. Carbonates, sulphates, and other oxidized ores of lead and zinc have long been worked by the Chinese, but are now nearly exhausted.

The author considers that the origin of the ores is to be attributed to the intrusion of granite masses into the ancient volcanic rocks, hot solutions having risen along shattered fault-planes previously produced and replacing rocks readily susceptible of mineralization, such as these rhyolitic tuffs would appear to be. The reserves of sulphide ores are very large, and after many vicissitudes the mines appear to be now in a flourishing condition, largely owing to the construction of a narrow-gauge railway 50 miles long, and bid fair to become one of the great zinc-lead producers of the world.

R. H. R.

IV.—Report on certain Minerals used in the Arts and Industries. V. Corundum. By P. A. Wagner. South African Journal of Industries, vol. i, No. 9, pp. 776–97. Pretoria, 1918.

OWING to the stoppage of the supplies of crude emery from Turkey and Greece the demand for the purer forms of corundum has greatly increased, especially as the better qualities are much more satisfactory in use, and are now very largely employed in munition-making. None of the artificial substitutes are found in practice to give such good results for the finer kinds of work and for specially hard materials. The output of South African corundum for the year 1917 amounted to 2,628 tons, so that the Union is now the largest producer of any country in the world. At the present time the corundum is mainly derived from surface deposits of one kind or another: gravels, more or less cemented conglomerates and boulders of disintegration: its original home is, however, undoubtedly in the gneisses and schists of the Swaziland series, where it is associated with a typical assemblage of metamorphic minerals, while the rocks are traversed by pegmatite dykes and veins: corundum is sometimes found in these also. The crystals are often very large, up to 10 inches in length by 5 inches in diameter. They are remarkably free from inclusions, and hence very pure samples can be obtained.

The chief corundum fields at present known are in the Zoutpansberg and Leydsdorp districts in the Northern Transvaal; some considerable deposits are also known in Little Namaqualand, while less important occurrences are numerous. This industry appears to have a promising future.

R. H. R.

V.—Flint Implements in Suffolk.

On some Human and Animal Bones, Flint Implements, etc., discovered in two ancient Occupation-levels in a small Valley near Ipswich. By J. Reid Moir. Journ. Roy. Anthrop. Inst., vol. xlvii, pp. 367–412, pls. xv, xvi, 1917.

The Ancient Flint Implements of Suffolk. By J. Reid Moir. Proc. Suffolk Inst. Archæol. and Nat. Hist., vol. xvi, pp. 1–38, 1917.

FOR two years Mr. Reid Moir had under close observation two well-marked occupation-levels in the deposits covering the sides of a small valley near Ipswich. A grant from the Percy Sladen Memorial Fund provided him with the requisite labour to make careful excavations. We now welcome his results, published in detail with numerous beautiful illustrations of the flint implements and other discoveries.

On the lower floor examined were found numerous bones of stag, roe deer, ox, pig, goat, and horse, besides some doubtful traces of mammoth and Irish deer. There were also three human bones, indistinguishable from those of modern man. The associated flint implements are of late Mousterian type, and there are also fragments

of very rough and primitive pottery. The flint implements from the upper floor are all Aurignacian, and two specimens from a still later deposit are clearly early Solutrean. The succession of the deposits in the valley near Ipswich is therefore the same as that already noted in the caves of France and Belgium.

Flint implements of all ages since the appearance of man have now been recognized in Suffolk, and Mr. Reid Moir has also published an interesting and useful summary of these, with excellent illustrations of the principal types, and a bibliography. We commend his paper to those who desire a clear elementary statement of the subject.

VI.—Geology of the Birmingham District.

The Downtonian of South Staffordshire. By W. Wickham King and W. J. Lewis. Proc. Birmingham Nat. Hist. and Phil. Soc., vol. xiv, pp. 90–9, 1917.

On Blattoid and other Insect Remains from the South Staffordshire Coalfield. By H. Bolton. Ibid., pp. 100–6, pl. vii.

Mammalian Remains in the Glacial Gravels at Stourbridge. By W. S. Boulton. Ibid., pp. 107–12, pl. viii.

THERE are three papers of special interest to geologists in the latest part of the Proceedings of the Birmingham Natural History and Philosophical Society. Messrs. King and Lewis have continued their researches on the Downtonian of South Staffordshire since their contribution to the Geological Magazine in 1912 (Dec. V, Vol. IX, pp. 437, 484), and they now publish a detailed summary of their results. They have found numerous fish-remains which determine the age of the deposits with certainty; and as these rocks are on the faulted fringe of a great coal-field, a knowledge of their precise sequence is of economic as well as of scientific importance. Mr. Herbert Bolton describes three wings of insects from the Coal-measures, probably of Coseley, one representing a new species of *Phylloblatta*, the others belonging to *Brodia priscotincta*. Professor W. S. Boulton gives an account of a sand-pit at Amblecote, Stourbridge, in which teeth of *Hippopotamus* have been found with remains of *Elephas primigenius*, *Rhinoceros antiquitatis*, and *Bison priscus*. All the bones and teeth are very fragmentary, but they show little evidence of having been rolled or water-worn. Careful search has been made for stone implements, but none have hitherto been identified. The spot is evidently well worth continued observation.

VII.—The Phylogeny and Classification of Reptiles. By S. W. Williston. Contributions from Walker Museum, vol. ii, No. 3, 1917.

PROFESSOR WILLISTON states that in this paper for the first time he ventures to express in tabular form his views as to the phylogeny and classification of the Reptilia. As he remarks, he has no startling novelties to offer, but nevertheless he makes a clear statement of the results of recent work which will receive the assent

of most palæontologists. The idea that in *Hatteria* we had the most generalized type of reptilian skeleton found among recent reptiles, and the belief that *Palæohatteria* was a member of the same group, for a long time misled most writers as to the interrelationships of the Reptilia. Cope, however, had already pointed out that the temporal region of the skull supplied the surest basis for forming an opinion as to reptilian affinities, and this idea was to a great extent carried out by A. S. Woodward. Osborn & McGregor made still more extensive use of these characters in drawing up their well-known system of classification. Professor Williston likewise employs the same characters, but has arrived at somewhat different conclusions from the two last-mentioned writers.

The main divisions he adopts are (1) Anapsida, (2) Synapsida, (3) Parapsida, (4) Diapsida. The first includes the Cotylosauria and the Chelonia, the former having no temporal opening in the skull and being probably directly derived from the same stock as the Stegocephalia. The second includes the Theromorpha, Therapsida, Sauropterygia, and Placodontia, in which there is a single temporal fossa which he believes arose from a separation of the jugal and squamosal. In the third, which includes the Ichthyopterygia, the Squamata, Proganosauria, and Proterosauria, there is likewise only a single temporal opening, which, however, is regarded as having been formed independently of the openings found in other phyla, the streptostylic type of skull found in the Squamata having arisen from the cutting away by the lower edge of an originally broad squamosal bar such as is found in the Permo-Carboniferous *Araeoscelis*. The fourth group, the Diapsida, includes the remaining reptiles, in which there are two temporal fossæ (lateral and superior), of which the lower one is regarded as the older, the upper having arisen as the result of a secondary separation of an orbito-squamosal arch. The introduction of the name Parapsida for a separate phylum including such, at first sight, dissimilar types of reptiles as the Ichthyosauria and the Squamata is the chief innovation in this paper, but the writer gives good reasons for its introduction, as he does for his other conclusions.

VIII.—Eocene Lizards in France. By G. A. Boulenger.

IN a recent note published in the Comptes Rendus of the Academy of Sciences, Paris (vol. 166, p. 889), Mr. Boulenger has shown that some of the genera of lizards (e.g. *Placosaurus*, Gervais, and *Plestiodon*, Filhol), from the Upper Eocene Phosphorites of France, belong to the family Helodermatidæ, which at the present day is represented only by the poisonous Gila Monster (*Heloderma*) of Arizona and Mexico and by *Lanthanotus* of Borneo. The determination of the relationships of these Phosphorite lizards was made possible by the discovery of a skull showing the rudimentary condition of the squamosal characteristic of the family. Mr. Boulenger is also able to state definitely that the dermal scutes described by Filhol under the name *Necrodasypus*, in the belief that they belonged to an Armadillo, are in fact cranial scutes of these Helodermatid lizards.

IX.—Nouveau genre des Musaraignes dans les dépôts miocènes de la Grive Saint-Alban (Isère). By Cl. Gaillard. Ann. Linn. Soc. Lyon, tom. lxii, p. 83.

IN this paper Professor Gaillard gives a very detailed description of the skull and mandible of an interesting new Insectivore (*Heterosorex delphinus*) which, while evidently referable to the shrews, shows some primitive characters, and in some respects approaches the moles, e.g. in the possession of a complete zygomatic arch and in the form of the fourth upper premolar. The skull is notable for its shortness and for the relatively great width of the cranial region. The mandible, so far as its posterior region goes, seems to be very similar in structure to that of *Neomys fodiens*, but is about one-third larger. The dental formula is as in *Crocidura*. The new form is said to be most nearly related to certain Asiatic genera of shrews, and also to have some resemblances with *Urotrichus*, a mole from East Asia. Probably it had undergone considerable modifications as a burrowing animal, but unfortunately the limbs are at present unknown. The last part of the paper consists of a careful comparison of *Heterosorex* with the fossil shrews hitherto known.

X.—Patagonian Geology.

The Problem of the Cretaceous-Tertiary Boundary in South America, and the Stratigraphic Position of the San Jorge Formation in Patagonia. By A. Windhausen. Amer. Journ. Sci. [4], vol. xlv, pp. 1–53, 1918.

IN California, Chile, and Patagonia it has been claimed that there is a gradual transition from the Cretaceous to the Tertiary faunas, with a remarkable mingling of types. Dr. Windhausen maintains that this is not the case, but that there is good evidence of a marked unconformity in all these regions. He describes and discusses the Upper Cretaceous and Lower Tertiary formations which he has examined in Patagonia, and concludes that they are distinctly separated both by a stratigraphical and by a faunistic break. He agrees with the brothers Ameghino that mammalian remains have been found associated with bones of Dinosaurs; but he considers that this association occurs in Palæocene, not in Cretaceous, deposits. Both sauropodous and theropodous Dinosaurs are met with in the Cretaceous of Patagonia, but only the latter range upwards into the Tertiary.

XI.—On the Crystallography and Nomenclature of Hollandite. By L. L. Fermor. Rec. Geol. Surv. India, vol. xlviii, pt. iii, pp. 103–20, 1917.

IN a former paper the author showed that hollandite is a crystalline mineral having the same composition as psilomelane. Further study of specimens from the original locality has shown that it belongs to the pyramidal class of the tetragonal system. It is also pointed out that the name romanéchite was applied by Professor Lacroix in 1900 to a mineral from Romanèche in France, which appears to be identical with or closely allied to hollandite.

XII.—Iron-ore Occurrences in Canada. Vol. II. By E. Lindeman and L. L. Bolton. Canada, Department of Mines. pp. 222 and 33 maps. Ottawa, 1917.

THE first volume of this publication has already been noticed in these pages; the second volume contains detailed descriptions of all the known occurrences of iron-ores within the Dominion. These include almost every known variety of ore, but the great majority of them seem likely to be of small commercial importance, at least under the present economic conditions.

XIII.—Special Reports on the Mineral Resources of Great Britain. Vol. III: Gypsum and Anhydrite, Celestine and Strontianite. Second edition. By R. L. Sherlock and B. Smith. pp. iv + 64. 1918. Price 2*s*.

THE second edition of this memoir is in the main a reprint of the first edition, but some further particulars have been given of deposits of gypsum in Nottinghamshire and Somerset, together with estimates of the reserves of gypsum still available in different districts.

REPORTS AND PROCEEDINGS.

I.—Geological Society of London.

1. *May* 15, 1918.—G. W. Lamplugh, F.R.S., President, in the Chair.

A lecture on "The Geology of the Italian Front" was delivered by Professor E. J. Garwood, M.A., Sc.D., F.R.S. The lecture was illustrated by lantern-slides, geological maps and sections, and tables of strata.

The President expressed to the Lecturer the thanks of the Fellows present.

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2. *June* 5, 1918.—G. W. Lamplugh, F.R.S., President, in the Chair.

(1) "The Kelestominæ, a Sub-Family of Cretaceous Cribrimorph Polyzoa." By William Dickson Lang, M.A., F.G.S.

The Kelestominæ are a sub-family of Pelmatoporidæ. The latter are a family of Cretaceous cribrimorph Polyzoa, whose costæ are prolonged upwards as hollow spines from the median area of fusion of the intraterminal front-wall. The broken ends of these spines form a row of pelmata (or, if small, pelmatidia) on the intraterminal front-wall.

The Kelestominæ are Pelmatoporidæ with an apertural bar each half of which is bifid; and the proximal and distal forks of each half are fused with the corresponding forks of the other half. The fused distal forks are also fused with the proximal pair of apertural spines, which are greatly enlarged.

The simplest known form of this arrangement is seen in the genus *Kelestoma*, Marsson. *Kelestoma* is characterized among the Kelestominæ by its great œcial length, and by the great number of costæ. *Kelestoma* has the following three species, which form a single

lineage: (1) *Kelestoma elongatum*, Marsson, with an incrusting asty; (2) a new species, with a bilaminar, erect asty; (3) *K. scalare*, Lang, with an erect, cylindrical asty. There is, in this series, a slight catagenetic decrease in the number of costæ, and the avicularian aperture becomes somewhat more pointed. The genus occurs in the Senonian, zone of *Belemnitella mucronata*, in the island of Rügen.

Morphasmopora, unlike *Kelestoma*, retains a small number of costæ and a short œcium; but the thickness of the proximal apertural spines, which are hardly recognizable as such, is enormously increased; the thickness of the bifid apertural bar is also increased. In *Morphasmopora brydonei*, Lang, there are four circum-apertural avicularia; and the proximal apertural spines and the apertural bar, though enormously developed, are not so large as in *M. jukes-brownei* (Brydone). The latter species has fewer costæ than the former, and but one pair of circum-apertural avicularia. There are also differences in the interœcial and interstitial secondary tissue of the two species. *M. brydonei* occurs in the island of Rügen and *M. jukes-brownei* at Trimingham; both from the Senonian, zone of *Belemnitella mucronata*.

(2) "The Geology and Genesis of the Trefriw Pyrites Deposit." By Robert Lionel Sherlock, D.Sc., A.R.C.Sc., F.G.S.

This pyrites deposit is worked at Cae Coch Mine, on the western side of the Conway Valley (North Wales), about 1 mile north of Trefriw.

A band of pyrites, about 6 feet thick, and of considerable purity, rests on the inclined top of a thick mass of diabase which is shown to be intruded into the Bala Shales that cover the ore-body. The shales immediately above the pyrites are shown by the graptolites contained to belong to the zone of *Nemagraptus gracilis*, and are the equivalents of the Mydrim Limestone of South Wales and of part of the Lower Cadnant Shales of the Conway Mt. succession: that is, they are near the base of the Bala Series according to the Geological Survey classification (Carmarthen Memoir, 1909). Northwards the intrusive is bounded by an overthrust mass of volcanic ash, which itself is cut off by an east-and-west fault against rhyolite, well seen in a roadside quarry and in the crags of Clogwyn Mawr. Intrusions of dolerite of much later age, probably late Devonian, or Carboniferous, are found in the rhyolite, and form the plateau above the mine, passing over shales, diabase, ash, and rhyolite in turn.

Pyrites deposits are classified by Beyschlag, Vogt, and Krusch in four groups: (1) Magmatic segregations, (2) formed by contact-metamorphism, (3) lodes, (4) of sedimentary origin. None of these modes of origin, however, will account for the Trefriw pyrites. The conclusion arrived at is that the diabase was intruded below a bed of pisolitic iron-ore. Hot water containing sulphuretted hydrogen given off from the intrusion, combined readily with the pisolites, which were in the form either of oxide or of silicate of iron, and formed pyrites. The graptolitic horizon at which the pisolitic ore occurs usually contains some pyrites, and this would be added to that derived from the above reaction. The pyrites was not formed by ordinary contact-metamorphism; because the intrusion is seen,

at places where the pyrites is absent, to exert only a slight hardening effect on the shale. In North Wales pisolitic iron-ore is known to occur in several places at the horizon of *Nemagraptus gracilis*. From the mode of origin assigned above to the pyrites it follows that the mineral is of Bala age, since it was formed before the intrusion, itself of Bala age, had cooled. The pisolitic ironstone must have been in existence in Bala times, and this supports the idea that the ironstone is a bedded contemporaneous deposit.

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3. *June* 19, 1918.—G. W. Lamplugh, F.R.S., President, in the Chair.

A lecture on "Some Features of the Antarctic Ice-cap" was delivered by Major Sir Douglas Mawson, D.Sc., F.G.S. In the course of his lecture, which was illustrated by a large series of lantern-slides, Sir Douglas Mawson said that the ice mantle of the south formally involved sub-Antarctic Islands, Patagonia, Southern New Zealand, and the higher mountains of Tasmania and of the neighbouring portions of Australia, but it retreated to its present confines—a circum-Polar Continent—at a time apparently concurrent with the disappearance of the extensive Pleistocene ice-sheets of the Northern Hemisphere.

The existence of a great land mass situated on the face of the globe just where the sun's rays fall most obliquely has the effect of intensifying the Polar conditions. This result is achieved by reason of the elimination of the ameliorating influence of the ocean and as a result of the acceleration of the circulation of the moist atmosphere from the surrounding sea to the land, owing to the wide difference in temperature pertaining over the one and the other. Thus the presence of extensive land at the Pole, in contradistinction to ocean, results, under present cosmical conditions, in increased refrigeration, and consequently in greater extension of the Polar ice-cap. This in turn reflects on the average temperature of other regions of the globe, for an ice surface absorbs but a relatively small proportion of the sun's radiant heat. The existence of the Antarctic Continent must therefore have some bearing on the climate of the Northern Hemisphere and be reckoned with as a factor contributing to the refrigeration thereof.

The lecturer laid great stress upon the work of the outflowing surface winds in developing the domed form of the ice-cap. These winds, owing to their persistence and violence, strip the surface of much of the newly fallen snow, and otherwise ablate the marginal zone, thereby considerably reducing the volume of ice that would otherwise reach the sea by glacial flow. Crevasses in the ice-cap observed far inland at "The Nodules" indicate that the ice of the hinterland is in motion.

In the seaward termination of the ice-sheet at Cape Denison, a basal zone, attaining as much as 50 feet in thickness, bearing englacial drift, is a well-marked feature.

The shelf-ice formations, including the Ross Barrier and the Shackleton Shelf were specially referred to: mention was made

of their growth and decline, of a method of determining their depth below water, and of the probability of specialized life existing beneath such formations.

The President expressed to Sir Douglas Mawson the thanks of the Fellows and visitors for his lecture.

II.—Mineralogical Society.

June 18, 1918.—W. Barlow, F.R.S., President, in the Chair.

W. A. Richardson: "On the Origin of Septarian Nodules." Septarian structure consists not of a simple combination of radial and concentric circles, but of irregular polygons closely simulating mud-cracking. By experiments with clay balls and films and comparison with timber cracks it was shown that radial cracks widening inwards are produced by internal circumferential contraction, radial cracks widening outwards by internal expansion, concentric cracks by contraction towards the centre, and polygonal cracks by either free or chemical desiccation. Moreover, analysis shows that septarian nodules are more aluminous towards the centre than the outside, and are therefore capable of contraction. The evidence disproved the expansion theories, and showed that contraction on numerous centres in a colloidal medium caused the cracking, and desiccation by chemical agents the contraction. The central portions are not merely enclosed clay, but clay that has undergone considerable chemical modification, and the original colloidal nature of the medium is so changed that closing of the cracks by absorption when placed in water cannot take place. Finally, the occurrence of the nodules suggests their origination by rhythmic precipitation according to the laws of Liesegang from solutions of bicarbonates diffusing through a colloidal medium.

Dr. G. T. Prior: "The Composition of the Nickeliferous Iron of the Meteorites of Powder Mill Creek, Lodran, and Holbrook." A simple and expeditious method of determining the amount and chemical composition of the nickeliferous iron of a meteorite was described. The method depends upon the use of dimethyl glyoxime for the separation of nickel. Its application to the meteorites of Powder Mill Creek, Lodran, and Holbrook gave percentages respectively of about 42, 30, and 6½ of nickeliferous iron, in which the corresponding ratios of iron to nickel were about 13, 11½, and 5.

CORRESPONDENCE.

MOUNTAIN BUILDING.

Sir,—The aim of my article in the Geological Magazine for May was to point out that those data for the earth's thermal condition and past history that agree best with evidence derived from totally different sources lead directly to an amount of compression of the earth's crust in cooling that is of the correct order of magnitude to account for mountain building. Mr. Deeley in his reply makes no attempt to answer this statement. What he does is to suggest that

different data, less satisfactory on other grounds, might lead also to a less satisfactory amount of contraction or even to an expansion. This is an argument in favour of the data and of the theory, and not against them.

His assertion that I would have readers "believe that the thickness of the radio-active layer has been fairly accurately measured", and his charge of "dogmatism", are definitely untrue. It was because it is not accurately known that I determined the available compression on two hypothetical distributions of radio-active matter, both permissible on other grounds, but widely different; the results they gave were not very different and were stated in the article.

I introduced no new theories regarding the properties of matter. What I did was to classify in a convenient way the known behaviour of different types of matter under shearing stress. The statement quoted from Maxwell that liquids and perhaps most solids are perfectly elastic as regards stress uniform in all directions is irrelevant to my discussion, which was explicitly limited to the differences between the stresses in different directions. In the light of present knowledge the account of shearing stress in Maxwell's book needs revision; for it makes no reference to elastic after-working or to the elasticity of such a substance as pitch, which in my classification would be a plastic solid with a very low limiting stress-difference. The common practice of regarding as a liquid a substance so elastic that tuning forks can be made of it is exceedingly inconvenient.

Had the conclusion, that my views on the solid and liquid states are quite inadmissible, been accompanied by the slightest argument, it might have been more impressive; or it might not.

HAROLD JEFFREYS.

THE PRE-THANETIAN EROSION OF THE CHALK.

SIR,—I have read with much interest the suggestive paper by Mr. H. A. Baker on the "Pre-Thanetian Erosion of the Chalk in the London Basin". I have for some time past been accumulating evidence for a similar study, but in 1915 wrote that "the evidence . . . is as yet too slight to allow of a definite map being made" (*Geology of Windsor and Chertsey*, Mem. Geol. Surv., p. 14).

Mr. Baker's map (Fig. 1) includes the area to which I referred, and appears to be based upon less evidence than that which my work for the Geological Survey had afforded. In the construction of such a map it seems natural to ascertain as far as possible the zone of the Chalk immediately underlying the Tertiary at the boundary of the latter, and to check the zones whose presence beneath the Tertiary is deduced from borings by these facts. This has not been done by Mr. Baker. The zone of Chalk on which the Tertiary rests has been ascertained by the Survey in the south-western part of the area shown on Mr. Baker's map, and a portion of the results has already been published (op. cit.). From the neighbourhood of Beaconsfield to the western margin of the map forming Fig. 1 he shows the base of the Tertiary as resting on the zone of *A. quadratus*. The fact is, that the zone is that of *M. cor-anguinum* at Beaconsfield, *Marsupites*

and possibly *quadratus* at Taplow, *cor-anguinum* again south and west of Taplow to the margin of the area, where *Marsupites* comes on again. In this part of the area, at any rate, Mr. Baker's zonal boundaries, deduced from borings, are in marked discordance with the facts ascertained and published.

I do not, however, wish to suggest that the method of deducing the Sub-Tertiary zones from boring records is useless; on the contrary, when the amount of evidence available is larger, the method may be of some value. The results obtained by Mr. Baker show that his evidence is insufficient, but that may be because he has apparently not made use of all the evidence available. For the benefit of those interested in the subject I may add a few points not mentioned in the paper I am criticizing; all are referred to in the memoir I have quoted, while the first was published in 1886. At Egham the Chalk Rock has been proved at a depth from the surface of 700 feet, or 346 feet from the top of the Chalk, suggesting the presence of *quadratus* zone; at Ottershaw the total thickness of Chalk is known to be 646 feet, suggesting *Marsupites*; at Windsor the Chalk is exposed below the Tertiary and probably belongs to the lower part of *cor-anguinum*.

From this evidence, combined with that referred to by Mr. Baker, I inferred that the plane on which the Tertiary rests "has been cut across a series of gentle folds whose axes run about E. 15° S." (op. cit., p. 14). I do not regard the above as more than a tentative solution of the problem, and it refers only to the southern half of the area mapped by Mr. Baker (Fig. 1), but I wish to point out that his conclusions must at any rate be regarded as "not proven".

I do not understand the suggestion on p. 299 that "the Streatham–Beckton fault is pre-Tertiary". It is certainly post-Tertiary, since it involves the Tertiary strata and dislocates the upper surface of the Chalk. Whether there was pre-Tertiary movement along the same line we have as yet no means of ascertaining.

C. N. Bromehead.

Geological Survey and Museum,
Jermyn Street, London, S.W. 1.
July 9, 1918.

OBITUARY.

WILLIAM LOWER CARTER, M.A., F.G.S.

Born August 9, 1855. Died June 19, 1918.

William Lower Carter was born at Stafford and educated at Derby School, where he distinguished himself in Natural Science. On leaving school he commenced work in a bank, but having a strong desire for theological studies he entered as a student at Springhill College, Birmingham, matriculating with first-class honours at London University. From Springhill he proceeded to Cambridge, having gained an Exhibition scholarship at Emmanuel College, where he again took up Science classes and passed the Natural Science Tripos Examination with honours, specializing in Geology. Leaving Cambridge he spent some time at the University of Halle in Germany,

and then returned to Springhill College for a final theological course.

In addition to his pastoral labours, he was ever keen on scientific research, and did some valuable original work. He was for many years also the Honorary Secretary of the Yorkshire Geological and Philosophical Society, editing its important journals and initiating efforts for the study of fresh fields in geology. He filled the office of Recording Secretary to Section C (Geology) of the British Association for the Advancement of Science, attending all the annual meetings.

WILLIAM LOWER CARTER, M.A., F.G.S.

In 1908 Mr. Carter accepted the important position of Lecturer in Geology and Crystallography to the East London College, a post which he continued to retain until the time of his death, also lecturing in Geography and Botany at various colleges and technical institutes in London. In this sphere he proved most successful, being an indefatigable teacher to whom preparation was never any hardship, and his pupils regard him not only with the esteem due to a careful instructor but also with affection. It was while lecturing on June 7 at Queen's College, Harley Street, W., that he was seized with cerebral apoplexy, from which he never rallied, but passed peacefully away on June 19, 1918, at his residence, 9 Belmont Road, Watford.

JOHN WATSON, M.A., F.G.S.

BORN 1842. DIED JULY 3, 1918.

THE death of Mr. John Watson, of Bracondale, Cambridge, deprives the geological world of a follower of the economic side of our science who possessed a very wide and full knowledge of the geology of building-materials.